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Psychological Bulletin

THE INVESTIGATION OF CONCEPT FORMATION[1]

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Despite the fact that concept formation has been the subject of numerous investigations, it is still poorly understood. The reasons for this lie, apparently, in two directions. First, the evolution of psychology has not gone far enough to free the treatment of concept formation from its past associations with epistemology and formal logic. Thus, terms like "abstraction" and "generalization" are still utilized—and still influence the nature of experiments—without sufficient analysis of the behavioral and genetic processes involved. Second, the data utilized in discussions of the subject are much too narrow, since they are usually drawn from limited experimental situations, and usually emphasize simple, readily quantifiable overt responses. The methods of investigation have usually been restricted in scope, and have lent themselves better to showing the results of concept formation than to revealing its nature.

It is the purpose of this article to review the research pertaining to concept formation towards the end of suggesting a broader basis for understanding it than has ordinarily been evident in experimental studies. The data available can be organized with reference to five major points. The first of these concerns the characteristics of a concept. Here one may inquire what kinds of facts must be taken into account in order to define a concept adequately. Much more than the verbal report of the subject is clearly involved, and a number of research studies have revealed internal and organizational, as well as behavioral, aspects of concepts.

The second point concerns the process (or processes) whereby an individual forms concepts. In this connection, two principal types of theories may be identified, one seeming to emphasize abstraction, the other generalization. It will be evident that neither theory is really complete, and it is likely that the two could meaningfully be combined.

[1] The writer is much indebted to Dr. Leslie J. Briggs for a critical and helpful reading of the manuscript.

It will further be shown that three different (but no doubt related) problems have been imperfectly recognized in the study of concept formation. An attempt will be made to formulate them and to relate the data to them.

The third point concerns the methodology of experiments on concept formation. As noted above, the methods employed have been surprisingly few in number; indeed, one may almost speak of established conventions in this respect. When the methodology is reviewed, it becomes evident that only two methods have commonly been used with children and three with adults.

The last two points to be taken up include a review of the experimental results with children and with adults. Research with children has mainly been directed towards a discovery of the characteristics of their concepts together with the conditions related to the development thereof, such as chronological and mental age, socio-economic status, etc. Research with adults has for the most part consisted of efforts to determine the effects of various conditions of presenting stimuli and of systematizing the behavior which is manifested in so-called concept-forming situations.

This article, therefore, attempts to organize the available research on concept formation in terms of these five major areas. In so doing it is to be hoped that a start can be made towards clarifying the problems involved in this important, but as yet inadequately investigated, area of psychological interest.

What Is a Concept?

None of the current definitions of a concept or of concept formation is entirely satisfactory (cf., as examples, English [9], and Smoke [58]). In view of the considerations to be raised later in the present article, it would be better not to cite any of the present definitions. Instead, we may summarize the general points which they cover. The essential features appear to be these:

1. Concepts are not direct sensory data but something resulting from the elaboration, combination, etc., thereof. This condition is easy to demonstrate experimentally by establishing a situation in which the subject cannot respond in a specified manner without data derived from previous sensory processes.
2. A corollary to the first condition, therefore, is that concepts depend upon the previous experience of the organism.
3. Concepts are responses which tie together, or link, or combine discrete sensory experiences. This condition may be demonstrated by showing that an individual responds to different stimuli in the same way.
4. It may be inferred that such ties or links are symbolic in nature; that is, the same response stands for a variety of data. In the human organism this

response is usually a word, and the word ties together different experiences with the same object, experiences with different objects somehow related to each other, the emotional responses aroused in these experiences, etc.

5. On the side of the internal processes of the organism, concepts represent selective factors. An external stimulus arouses a symbolic response, on the one hand, or a symbolic response guides perceptual response, whichever comes first. For example, a creature running along a road may evoke the response "dog"; or a boy scout may go out on a search for "birds." In experiments on concept formation, both kinds of selective mechanism are clearly revealed. Thus, an experimenter may present a whole series of objects (or a group of objects simultaneously) and ask the subject to classify them; or he may instruct the subject to find all the round objects.

In addition to these five general characteristics of concepts, various studies have suggested important aspects of how a particular concept may be organized in the mental structure of the individual.

Hayakawa (**23**) has pointed out that concepts have both *extensional* (or denotative) and *intensional* (or connotative) meanings. The extensional meaning is "something that *cannot be expressed in words* because it is that which words stand for" (**23**, p. 61). The intensional meaning is "that which is *suggested* (connotated) inside one's head" (**23**, p. 62). Whenever words are used to symbolize concepts, therefore, both extensional and intensional data may be involved.

The experiments of Reed (**52**, **53**, **54**) suggest that another way in which concepts are organized is in terms of their *consistency* or "correctness." His subjects were required to learn, by the anticipation method, the names of cards. Each card contained a series of four words, one of which fell into a category symbolized by a nonsense name. Under these conditions, Reed discovered that two kinds of concept might be formed. A *consistent* ("correct") concept was one achieved by classifying the words into groups and then attaching to the group a nonsense syllable which worked (i.e., made it possible to identify the card). On the other hand, an *inconsistent* ("incorrect") concept was one achieved by attaching a nonsense syllable to some group of unrelated words, such as the first word in each group. Both achievements must be regarded as concept formation regardless of how "objectively correct" the concept may be judged.

Although this distinction is based upon a fairly narrow experimental situation, it warrants further development. Whatever the relationship between inconsistent and intensional aspects of concepts, the two characteristics are not the same. Thus, Reed's experiments show that an inconsistent concept may nevertheless have an extensional meaning (the subject can use it to name a card correctly). Furthermore, it is quite likely that in everyday life concepts are never completely consis-

tent but have their inconsistent components dependent upon accidental groupings of objects, inaccurate perceptions, etc.

Another way in which concepts are structured has been outlined by Welch (**68, 70, 71**). He has shown that they have both *horizontal* and *vertical* organization and that, in consequence, the same object (or relation) has different points of reference depending upon the other objects with which it is compared. Horizontally, objects may be classified into different categories, all of them equally inclusive. At the same time, objects may be classified vertically, into groups of varying complexity, or into more and more inclusive categories. In a given situation, the horizontal classifications are less fixed and more dynamic than the vertical. Nevertheless, even the vertical levels may be defined differently in different situations.

These important and neglected features of concepts may be clarified by means of an illustration. Figure 1 shows a collection of stimuli.

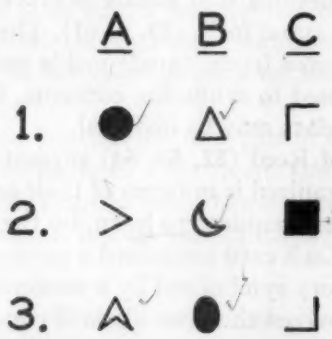

FIG. 1. A COLLECTION OF STIMULI WHICH MIGHT BE USED IN AN EXPERIMENT ON CONCEPT FORMATION.[2]

If someone were asked to select those that belong together, he would have several alternatives. Let us say that he is asked to point to the curved figures and responds by touching A-1, B-2, and B-3. This response corresponds to the *extensional* aspect of the concept. Suppose that he comments, "I dislike round objects because they're so indefinite in shape." This behavior corresponds to the *intensional* aspect of concepts.

A concept of curvedness which resulted in the selection of A-1, B-2 and B-3 would be *consistent*, whereas one which resulted in the selection of A-1, A-2, and A-3 would be *inconsistent*.

[2] This figure was very kindly drawn for the writer by Professor L. Scott Daniel.

In Figure 1, ***horizontal*** concepts would be those which resulted in the selection of A-1, B-2, and B-3, of A-2, C-1, and C-3, and of A-3, B-1, and C-2 as three classes of objects; another horizontal arrangement would be the choice of A-1, B-3, and C-2, of A-3, B-1, and B-2, and of C-1, A-2, and C-3.

A ***vertical*** arrangement might be as follows:

First Hierarchy —Each figure as distinct from each other figure (i.e., A-1 is a filled circle, B-1 is an empty triangle, etc.).

Second Hierarchy[3]—Group X. A-1, B-2, B-3.
Group Y. A-3, B-1, C-2.
Group Z. A-2, C-1, C-3.

Third Hierarchy[3] —Group O. A-1, A-3, B-1, B-2, B-3, C-2.
Group OO. A-2, C-1, C-3.

Fourth Hierarchy —All of the figures taken as a complex group (i.e., they are all plane figures).

Finally, note that any single figure may be related to any of the others in many different ways depending upon the concept involved. Horizontally, A-1 may be "curved," or "solid," etc. Vertically, in the second hierarchy, A-1 may be regarded as a "curved" figure, in the third hierarchy, as a "completed" figure, etc. Also consider A-3. In the third hierarchy it may be regarded as a "completed" figure from one point of view but as an "incompleted" from another.

Putting together all of these considerations, then, it appears that concepts should be placed in a broader, more dynamic context, than has been the case up to now. They must be regarded as selective mechanisms in the mental organization of the individual, tying together sensory impressions, thus aiding in the identification and classification of objects. But concepts involve more than the integration of sense impressions, against the background of which recognition occurs, for they are linked with symbolic responses which may be activated without the physical presence of external objects. That is, concepts can be given names—can be detached from specific instances, by means of a word—and used to manipulate experience over and beyond the more simple recognition function. The symbolic response, however, stands for whatever it has been linked with in the previous experience of the organism and depends upon how that past experience is organized. Thus, a symbolic response does not have a fixed, permanent meaning but represents a momentary focussing of experience upon a particular stimulus situa-

[3] Note that various horizontal classifications are possible at each hierarchical level.

tion, combining intensional as well as extensional data, inconsistent as well as consistent relationships, and placing the stimulus in a context of greater or lesser inclusiveness.

What Is Concept Formation?

The next question which arises has to do with the process of forming concepts. It is usual to distinguish, here, the activities of abstracting and generalizing. The former signifies the linking of one sensory experience to another so that some details are left out and others become dominant; in this sense, the concept is a response which stands for these dominant details. The latter term signifies that the dominant detail (or group of details) resulting from abstraction is used as a basis for responding similarly to the separate objects linked by abstraction and for responding to other objects similarly linked.

The two most clearly stated theories of concept formation depend upon the distinction between abstraction and generalization.

1. *Composite photograph theory.* The primary emphasis in one theory is on abstraction. As Woodworth states it, "the features common to a class of objects summate their impressions on the observer, who thus gradually acquires a picture in which the common features stand out strongly while the variable characteristics are washed out" (**76**, p. 801).

This view, in some form, appears to have predominated among psychologists who have written on the subject. It has been explicitly stated by Moore (**43**), Fisher (**12**), Hull (**37**), Kuo (**38**), and Stevanović (**61**) and is at least implicit in the interpretations of Thorndike (**62**) and others. The "spectator" behavior described by Heidbreder (**26**) belongs in the same category.

Smoke (**58, 59**) has criticized this theory on the grounds that the term "common features (or elements)" is misleading. He suggested that it is not specific components common to a series of stimulus patterns which summate in the concept, but relationships between them. However, he appears to be in substantial agreement with the theory.

2. *Active search theory.* By contrast, another theory emphasizes generalization rather than abstraction. Here, as Woodworth states it, "The concept is supposed to originate as a hypothesis, which *O* proceeds to test by trying it on fresh specimens of the class" (**76**, p. 801). This second view, therefore, assigns to the individual a more active, deliberate role than the first theory. It seeks to make of concept formation a more orderly, controlled activity than does the other view.

This theory has seldom if ever been advanced as the sole explanation of concept formation, partly, perhaps, because of the strength of the earlier theory, which has such apparently great logical consistency and sanction. Perhaps of more significance than the condition of beginning with a hypothesis is the factor of active search. Many experiments have shown that, at least in the adult, concept formation may be the result of controlled and effortful search. Thus, English (**10**) found that some of his subjects, in the process of abstraction began with a deliberate intention to analyze. Heidbreder (**26**) observed in "participant behavior" that the subject was responding with conscious and

definite effort. Smoke (58) stresses the active processes of concept formation, and Reed's experiments (52, 53, 54) similarly show the influence of set and effort.

It is better to draw these two theories together rather than to make a choice between them, since experiments show that abstraction and generalization are often difficult to distinguish in the actual behavior of the organism. Under some conditions the individual may, in fact, be essentially a passive recipient of sensory impressions which gradually summate into the concept. Under other conditions it may be that an individual proceeds by establishing an hypothesis and then deliberately checking it against instances. More than likely, they are mutually complementary approaches to a situation and occur as interrelated processes. This conclusion is especially warranted with respect to concept formation in adults. It cannot be assumed that the adult subject is really acquiring a "new" concept; instead, he is learning to apply concepts (or aspects of concepts) already in his possession in some unusual or systematic fashion; i.e., he is solving a problem rather than learning a concept, in contrast to a small child who is more likely to be learning something new. In this sense, therefore, both abstraction and generalization, for the adult, are far more conscious, controlled, motivated activities than they seem to be in the child. Hence, for the adult, the distinction between abstraction and generalization breaks down. One cannot say that, for the adult, the sequence is perception-abstraction-generalization except from the logical standpoint. In a child, on the other hand, confronted with an entirely new realm of experience, such a sequence appears to be more likely.

Heidbreder's findings with respect to a complex concept formation situation (26) bear out the view that processes of both kinds operate at the adult level. She shows that concept formation involves, on the one hand, "analysis" and on the other hand, "synthesis." The subject both seeks and obtains information about the situation and also organizes his activities in relation to it. "Participant behavior" and "spectator behavior" do not occur as mutually exclusive methods of approach, although one or the other may at times predominate. They both contribute to the end result, the formation of a concept.

Problems in the Study of Concept Formation

These preliminary considerations lead to a recognition that several different problems are actually involved in the investigation of concept formation and that investigators have not clearly distinguished between them. These fundamental problems may now be stated. For convenience, identifying names are given to them.

Problem 1. Ability to conceptualize. This question may be stated explicitly in this way: How can one explain and describe the development in the child of the ability to form and use a concept? This problem is clearly a genetic problem and should be recognized as such. It cannot be answered, in all probability, by studying adults and then extrapolating from their behavior to early stages of child development. In any case, this problem is a general one and represents an effort to trace, with age, the unfolding and elaboration of a general function in the behavior of the individual together with the conditions which influence that development.

Problem 2. Acquisition of concepts, or repertory. This question may be formulated as follows: What concepts—or patterns of concepts—characterize various stages in the development of the child's thinking and acting? This is, so to speak, an educational problem. It may be that it is inextricably bound up with Problem 1, but this writer, at least, is not prepared at the present time to regard it as the same thing. The reason lies in the fact that the emphasis in Problem 2 is more specific than that in Problem 1; i.e., the concern in Problem 2 is with the particular concepts which the child possesses and with the way he utilizes them. Hence, a number of additional questions arise pertaining to the order in which different concepts are acquired, how complex they are, how many concepts the average child of a given age possesses, etc. Clearly, this problem, like Problem 1, must be answered by tracing the learning process in children, although related questions have meaning also in the study of adults.

Problem 3. Achieving a specific concept. This question may be stated in this manner: How does the individual go about attaining a particular concept? What behavior is manifested, and what conditions influence that behavior? Although this third question is pertinent to children, too, it has primarily been investigated at the adult level. In fact, when adult subjects are used it appears to be inevitable that emphasis is placed on this problem, because the adult has already developed an ability to conceptualize and has already acquired an enormous repertory of concepts. It is probable, therefore, that the adult does not typically acquire new concepts so much as he applies concepts which he already possesses—or learns new variations, hierarchies, etc., of those concepts.[4]

Experimental Approach: Methodology

In attempting to subject the problems of concept formation to standardized, objective conditions of observation, investigators have exercised a considerable degree of ingenuity. The stimuli which have been devised are clever, interesting, and novel; several excellent series of studies have been conducted; nevertheless, most of the experiments have conformed to a very few relatively simple patterns. In fact, only three basic methods have been used with adult subjects and two with children.[5] Some of the studies have employed two or three of these

[4] Cf. Hebb (25), who goes so far as to say that there are two kinds of learning, one characteristic of the child, the other of the adult.

[5] The emphasis here is on laboratory research. The methodology might be extended if one included relevant data from other fields such as intelligence testing or therapeutic situations.

methods in such a manner that the distinctions to be drawn below are somewhat arbitrary.

Experiments with Children

The interview-questionnaire method. This approach has been employed mainly with reference to Problem 2 formulated above. Typically, the investigator works out a procedure for obtaining from the child his viewpoint or judgment about the nature of reality, morality, causal relations, life, death, etc. Piaget (**49, 50, 51**) is the best-known exemplar of this method (he calls it a "clinical method"). The procedure is essentially that of questioning the child in an objective manner about natural and psychological phenomena, striving to obtain from the child his own interpretations thereof. Obviously, there are many pitfalls in this approach, but Piaget has been patient and careful in his efforts to objectify his questions and his interpretations of the answers.

Attempts to verify Piaget's findings have modified the method in the direction of greater objective standardization, Thus, the questions have been administered in questionnaire form, or each child has been asked the same questions in the same way (e.g., **3, 6, 7, 46, 56, 57**). Sometimes the investigator has actually demonstrated natural phenomena, then asked the child to explain them (**7, 46**).

The performance method. The second kind of approach has been used primarily to investigate Problem 1.[4] Here, the investigator places the child in a situation which involves the learning or use of one or more concepts and observes the child's behavior. Thus, a child is taught that if he opens a box on the front of which is a triangle he will find a piece of candy. He is then presented with two boxes, one of which is marked with a triangle, the other with a square, to see if he can discriminate between them (**15, 16, 44, 65, 66, 67, 68**).

This second method has distinct limitations from the standpoint of interpreting the child's behavior. Thus, even if a child responds correctly (that is, appears to act in terms of the "concept") it is difficult to be sure that the response is not a simple conditioned response. In the training series the triangle→candy relationship is set up; in the discrimination series there may be no reason to suppose that the response is any more complicated than that (i.e., the square might simply be ignored). Care must be exercised, therefore, not to assume that the child's behavior is more complex than it really is. This difficulty can at least partially be reduced by emphasis on adequate experimental controls (such as by varying the stimuli) and by restricting interpretation as much as possible to the actual behavior of the child. With children old enough to verbalize, introspective data might have considerable value in supplementing the behavioral data.

Experiments with Adults

The introspective method. The essential characteristic of this method is that the subject is exposed to a series of stimuli and asked to analyze and report the mental processes resulting from the stimuli. The emphasis is upon the acts or processes in the individual which are not directly accessible to others. The method at the present time is mainly of historical interest. Early investigations of concept formation made use of it (**10, 12, 43**). Thus, a series of drawings might be exposed tachistoscopically until the subject believes that he has discovered something important about the stimuli. The exposures are stopped while the subject reports his mental processes. Experiments in which the emphasis has been on behavioral data have also included introspective reports (e.g., **26, 52, 53, 54**).

It might be observed that valuable data can be obtained through introspective studies and that this method should not be abandoned too hastily. For example, studies of the intensional and inconsistent aspects of concepts might well utilize it.

The learning method. Here, the subject is placed in a situation which requires that he learn some clue or principle by means of which he can identify the members of a class. Typically, the conditions have resembled a memorizing experiment. Thus, the subject is first taught the names (usually, nonsense syllables) of a series of stimuli, say by the anticipation method. He is then shown a second series of stimuli, each of which has something in common with the first series, to see if he can correctly name them (**26, 28, 37, 52, 61**).

The problem-solving method. The third approach is similar to the learning method except that the subject does not need to go through a preliminary learning period. He is usually presented with a collection of stimuli, either simultaneously or successively, and asked to determine what they have in common or to classify them in some way. Thus, the subject may be told that a series of drawings are all instances of the same concept; he is to examine them and explain or define the concept (**58**). Another variation is to present the subject with a collection of objects, drawings, or cards and ask him to select those that belong together (**2, 19, 20, 21, 22, 34**).

Concept Formation in Children

At the present time it would be presumptuous to suppose that we possess enough knowledge to present a complete and accurate account of concept formation in children. There is a considerable and growing body of data dealing with that and other aspects of the intellectual development of children, but so many questions regarding interrelationships and attendant conditions remain to be answered that it would be fruitless to attempt to draw all the data together. We shall, then, con-

centrate on the investigations which are directly pertinent to concept formation. These fall naturally into two major groups, those which purport to specify the characteristics of children's concepts and those which attempt to define the conditions related to the formation of concepts, notably age. In these studies, the three problems set forth earlier are not clearly differentiated. Thus, it is not always easy to tell whether the results are of general importance with respect to the development of the ability to use concepts ("conceptualizing") or merely with respect to how a particular concept is achieved and used by the child.

Characteristics of Children's Concepts

The work of Piaget (**49, 50, 51**) set the stage for a host of subsequent investigations. Using the method briefly described above, he obtained a wealth of provocative material relating to how the child conceives of himself and his surroundings. Perhaps the two major features of Piaget's interpretation of his results are (1) that the child's thinking is essentially different from that of the adult and (2) that the child's concepts pass through a series of rather clearly defined stages each of which is typical of a particular age-group.

According to Piaget, the child's thinking is characterized by *realism*, *animism*, and *artificialism*. By realism, Piaget means "a spontaneous and immediate tendency to confuse the sign and the thing signified, internal and external, and the psychical and physical" (**49**, p. 124). In other words, the child does not distinguish between his own experience and external reality. There are four stages in the evolution of realism, as follows (**49**, p. 126):

1. Absolute realism: "during which no attempt is made to distinguish the instruments of thought and where objects alone appear to exist."
2. Immediate realism: "during which the instruments of thought are distinguished from the things but are situated in the things."
3. Mediate realism: "during which the instruments of thought are still regarded as a kind of things and are situated both in the body and in the surrounding air."
4. Subjectivism or relativism: "during which the instruments of thought are situated within ourselves."

In general, the child is alleged to pass from a confusion of self and world to a distinction between them. For example, Piaget gives protocols relating to the notion of thought, the nature of names, and the nature of dreams. The child first confuses thought with the object thought about and only much later reaches the point of recognizing the difference between the thing, or the word, and the thought. With respect to names, the child passes from a stage of assuming that the names of things are in the things themselves, to a stage of recognizing that names are in the voice, the head, or the thought itself. Dreams are first

regarded as coming from outside and remaining external, then arising in ourselves but external to us, and finally are conceived to be internal in origin and to remain internal.

Piaget means by animism in children's thinking, "the tendency to regard objects as living and endowed with will" (49, p. 170). He postulates four stages of development in this dimension:

1. At first, the child believes that everything active is conscious. "Thus a stone may feel nothing, but if it is moved, it will feel it" (49, p. 174).
2. A second stage is one in which consciousness is attributed to things that can move, such as the sun and moon, clouds, rivers, bicycles, etc.
3. In the third stage, consciousness is attributed to things that can move of their own accord. Thus, a distinction is drawn between movement exhibited by the object itself and that introduced by an external agent; the sun and wind are conscious, but not the bicycle.
4. Finally, consciousness is restricted to the animal world.

A third general characteristic is artificialism: "regarding things as the product of human creation, rather than in attributing creative activity to the things themselves" (49, p. 253). Thus, for example, the origin of the sun may first be explained in terms of some human agency (e.g., it was lighted with a match), and then by intermediate stages the child's thinking progresses to the point where it is recognized that human activity has nothing to do with the phenomenon.

In presenting his findings with respect to many different concepts, Piaget defines the successive stages through which that particular concept passes, and links them with age. For example, he alleges that the four stages of animism occur, on the average, at ages 4 to 6, 6 to 7, 8 to 10, and 11 or older. In the case of physical causality (50), he finds that some seventeen types of explanation are advanced, falling into three main stages. In the first, up to about ages 5 to 6, the child's explanations are "precausal," representing a confusion between psychological and mechanical factors. In the second, at about ages 7 to 8, children explain natural phenomena in ways which are still essentially "precausal," but the magical types tend to disappear. Finally, in a third stage, which is completed at about ages 11 to 12, more rational forms become dominant.

Thus, Piaget has found in many different areas the development in children of concepts which pass from diffuse, prelogical, subjective forms to more differentiated, logical, and objective forms. He claims that general stages are characteristic of particular ages, and that concepts of a particular kind (e.g., of "life") evolve in a definite manner.

Nagy (45) obtained results similar to Piaget's from a study of children's theories of death. She obtained data consisting of compositions, drawings, and discussion from 378 children. Three stages were found, as follows: (a) denial of death as a regular and final process; i.e., everything including the dead and lifeless is considered to be alive—the ani-

mism described by Piaget (ages 3 to 5); (b) death is personified, considered a person—essentially Piaget's artificialism (ages 5 to 9); and (c) death is understood as a process and as inevitable (ages 9 and older).

Some rather serious criticisms of Piaget's views have been given. Hazlitt (**24**) has suggested that his claims rest upon two unwarranted assumptions, namely: (a) that thinking can be identified with ability to express ideas verbally; and (b) that the content of thought is irrelevant—that is, if a child cannot understand or state a relation in regard to one subject matter, he cannot do so in regard to another. She herself conducted experiments designed to have the child behave in terms of a concept rather than merely to verbalize it. She concluded that children display a grasp of relations very early and that the only limitation is lack of experience. In general, an assumption that there is a qualitative difference between adult and childish thinking is not warranted. Deutsche (**7**) has done a very careful study of children's concepts of causal relations, as a result of which she criticizes Piaget's classification of answers into seventeen types. In the first place, she found that the definition of the types is so vague and indefinite that it is difficult to classify answers. In the second place, the types are of extremely unequal value, some being too inclusive, some extremely narrow, although Piaget tends to treat them as though all were characteristic of children's concepts. Finally, it is a matter of personal judgment as to what a child really means by his explanations. Many criticisms brought to bear against Piaget rest upon his methods, which are indeed difficult to accept fully; for example, one never knows how many subjects he is relying upon in the formulation of his conclusions and one never knows how many children were asked the same questions. Other criticisms depend in part upon Piaget's terminology, although he himself admits the fallibility of such words as "animism" and "realism."

A large number of studies, however, have been carried out with more standardized procedures than those of Piaget, some of which have confirmed his results, others of which have disagreed with them. Grigsby (**18**) investigated developmental trends in concepts of time, space, cause, part-whole relations, discordance, and number, using children between the ages of 2 years 8 months and 6 years 4 months. In each of these kinds of concepts she found gradations, although these were more related to mental age than to chronological age. Specifically, with respect to causal relations, she found good agreement with Piaget's seventeen types.

This is in marked contrast to the results of Deutsche (**7**), already mentioned, whose experiment is in many ways a model of research in this field. She developed two tests, both concerned with causal relations, the answers for which were written. In the first test there were eleven demonstrations of physical phenomena which the child was asked to explain. For example, the experimenter placed a jar over a lighted candle

and then asked, "Why does the candle go out?" In the second test there were twelve questions, such as, "What makes the wind blow?" and "What makes the snow?" She used 732 subjects (thereby meeting at least one criticism of Piaget) distributed rather equally over the age range 8 to 16—both boys and girls. They were also distributed over Grades 3 to 8 and corresponded well with the percentage of the male population of Minneapolis falling into seven occupational groups. I.Q.'s ranged between the intervals of 60–69 and 140 and above. The enormous number of answers obtained were carefully and objectively evaluated in several ways by several judges (in one instance by thirteen judges, in another by three). The following are her major conclusions pertinent to the characteristics of children's concepts of causal relations:

Classification of all the answers to Piaget's seventeen types of causal thinking revealed that only four types of thinking (phenomenistic, dynamic, mechanical, and logical deduction[6]) are found in large enough frequency to warrant further analysis.

No evidence was found that children's reasoning develops by stages. Both quantified and qualitative analyses show a gradual progression in answers with advancing age, and all kinds of answers are found spread widely over the age range.

Evidence was found of specificity in children's causal thinking as opposed to a general level of thinking. The child responds to different questions with answers differing in qualitative as well as quantified value.

However, Deutsche's study itself has certain limitations. Perhaps the most important of these is the nature of her questions. They were not as varied as those of Piaget. In fact, the absence of some types of answers in her data might be accounted for by the difference in the kinds of question which she asked. Another limitation has to do with the ages of her subjects, the youngest of whom was eight. By this age, according to Piaget, some of the precausal types of answer have largely disappeared. As a check on this point, Deutsche administered the tests to thirteen kindergarten children (five years old). Despite the fact that these data are not presented in the same way as those for the other subjects, they seem to agree fairly well with Piaget's formulation. That is, the younger subjects are much lower in total score, and the precausal types of answer appear more frequently. Nevertheless, even at the five-year level, the same general range in type of answer is evident as in the older levels.

Russell and Dennis (56) devised a standardized procedure for investigating animism in children's concepts. It consisted of a questionnaire relating to twenty objects, some physically present, some visible

[6] *Phenomenistic:* two facts given together in perception, no relation except contiguity in time and space; *dynamic:* no animism, but still sees in objects forces capable of explaining their activity and movements; *mechanical:* explanation by contact and transference of movement; *logical deduction:* explanation by the principle of sufficient reason.

outside the room, and others presented verbally. They tested 385 children ranging in age from 3 years to 15 years 6 months. Criteria were set up whereby the answers could be classified into five categories, the four stages previously formulated by Piaget and a "No Concept Stage." This method was then used to check Piaget's contention that these categories of response are characteristic of definite ages (**57**). The subjects numbered 774, ranging from Grade 1 to Grade 8. It was found that 98.5 per cent of the subjects could readily be classified into one of the stages. Curves based on the data showed increasing modes with increasing mental age, from which result Russell concludes that it is probable that individuals pass sequentially through the stages. Nevertheless, as Deutsche had found, for causal concepts all the stages occurred at every age, rather than being distinct from each other as Piaget had described them.

Dennis (**6**) also applied Piaget's questions to Zuni and Navaho children. He found that animism, realism, and artificialism are also typical of their concepts. Bruce (**3**) similarly investigated the occurrence of animism in Negro compared to white children. She found Piaget's four stages, but she believes that the concept of "alive" is a more meaningful way to interpret children's beliefs than to refer them to "animism." Like other investigators, she found that all four stages occur at all age levels.

The experiment by Oakes (**46**) tends to agree with Deutsche. His method was patterned closely after Piaget, and he attempted to obtain age sampling at younger ages than did Deutsche. Thus, he had 77 kindergarten subjects, from age 4 years 10 months to 7 years 4 months, in addition to smaller numbers of children in Grades 2, 4, and 6. He used fifteen kinds of questions ("How were the hills made?") and seventeen simple experiments similar to those of Deutsche. He found, as Deutsche did, that Piaget's seventeen types of explanation are not usable; instead, he classified the answers into three major categories, physical or materialistic, non-physical or non-materialistic, and failure to explain. For both types of tests there was a marked increase in the incidence of physical explanations with age, accompanied by a corresponding decrease in the other categories. In his results, too, all types of answer were given by all the age groups, and each subject gave a wide variety of explanations. Hence, he found no evidence for Piaget's contention that definite stages characterize definite ages. "The types of answers given by these subjects were influenced by the nature of the problem . . . " (**46**, p. 93). The great majority of the answers were naturalistic, which contrasts with Piaget's emphasis on various kinds of magical, illogical, etc., explanations.

Finally, it should be noted that experiments of very different kinds have also cast doubt upon Piaget's view of age-stages in concept formation. For example, Welch and Long (**42, 70**) tested children between the

ages of 42 and 83 months with tasks which required the use of concepts previously learned. Although the number of subjects who could pass the tests increased with age, there was no orderly progression in this respect.

One general conclusion to be drawn from these studies is that children's concepts seem to change with increasing age but that the change is more in the nature of a gradual progression (towards greater adequacy or informed understanding, perhaps) than of definite stages. This change does not occur at the same rate for all children. Indeed, some may never achieve the more advanced kinds of explanation at all, since even adults display concepts similar to those of children. Further, no child at a particular age gives consistent responses of one type or another but instead may give many different types, depending upon the situation.

Another conclusion to be stated tentatively is that the apparent difference between children's concepts and those of adults is less a matter of kind than of degree. Although animism, artificialism, and realism —to retain Piaget's terms—are found relatively more often in children, these characteristics are not absent by any means from the thinking of adults. The reverse is also true, since the supposedly more advanced, more logical, more correct characteristics occur in the concepts of children. As will further be shown in the following section, it is probable that Hazlitt's observation is correct: the greatest factor in the difference between concepts of children and adults is that the former have less experience and knowledge (**24**).

A further observation which follows from this has been emphasized by Wolff (**75**). It is necessary, in order to understand the concepts of children, to judge from the child's standpoint, not that of the adult. Thus, what strikes the adult as "illogical" may not be so from the child's outlook, which is merely based upon less, or different, experience.

Conditions Related to Concept Formation in Children

In this second area of research much remains to be done—for example, in terms of relating what is known about development specifically to concept formation. We shall merely indicate some of the possible conditions which have emerged in experiments dealing with concept formation as such.

Age. Welch and Long (**42, 68, 70, 71**) have shown that the conceptualizing ability of children seems to develop from simple to more complex levels. A pre-abstract period leads gradually to an ability to grasp first-hierarchy concepts, such as that "men" and "women" are all "people." This period begins at about the twenty-sixth month and is followed, by about the middle of the fourth year, by the ability to grasp second-hierarchy concepts ("potatoes" are "vegetables," "apples" are "fruit," and both "vegetables" and "fruit" are "food"). In turn, the

child may develop an ability to grasp concepts of still higher hierarchies in later years. It should be noted, in relation to the preceding section, that at least after the age of eight years children can deal with concepts at any of the lower hierarchical levels (**42**).

Reichard, Schneider, and Rapaport (**55**) have used sorting tests to study the conceptual abilities of children. They suggest that there are three levels of development: (1) *a concretistic level*, where classification tends to be made on the basis of non-essential incidental features of the objects; (2) *a functional level*, where classification is on the basis of use or value, etc.; and (3) *a conceptual level*, where the child classifies more nearly on the basis of abstract properties or relations. The concretistic performance was more typical of the youngest subjects (up to 5 or 6 years), and the functional performance more of older children (up to 8 or 10 years).[7]

Still another change in conceptualizing with age is suggested by the work of Colby and Robertson (**5**), who tested three age-groups (3 to 6, 6 to 7, 7 to 9) with colored forms and retested them after one year. There was a progressive shift towards a dominance of form as the basis for classification. Welch (**66, 68**) reports a similar result, although he concludes that form tends to be dominant below the age of 3 years, with a shift to color between 3 to 6, and a return to form again after age 6.[8]

Other efforts to trace conceptualizing in relation to age may be summarized as follows:

Triangularity and other forms (**15, 16, 44**): concept can be established at 15 months to 2 years.

Roundness (**40**): concept established as early as age 3.

Magnitude (**36, 63, 65, 66, 67, 69**): simple concepts of size can be established quite early, perhaps at the fourteenth month, but "middlesizeness" comes later, probably not much earlier, on the average, than the third year.

Contradictory relations (**8**): this situation is one in which children are asked to discriminate the larger of two equal ring-segments so placed that one gives the illusion of being larger than the other; the larger is then placed so that it appears smaller. Recognition of the contradiction was found to appear between ages 3 and 4, the same time as the concepts "big" and "little."

Time (**1, 13, 14**): concepts in this area undergo long and continuous development. General divisions of time are grasped early, "morning" or "afternoon" by 4 years, what day it is by 5 years, the conventional time-system by the sixth grade in school. Still other time concepts are grasped later, with adults about equal, on the average, to ninth- or tenth-graders.

Cause-effect relations (**39**): grasped by the eighth or ninth year but probably not before.

Social concepts (**47**): there is a steady increase from grade to grade although concepts in different problem areas differ. Recognition of social problems is not great before the sixth or seventh grade.

[7] See below for discussion of concrete and conceptual attitudes in adults.

[8] Apparently, form also tends to be dominant over color in adults, as would be expected from the above (see **31**).

Intelligence. It is probably safe to say that psychologists have assumed that intelligence and concept formation are related, although they have not yet worked out the relationship explicitly. There are two assumptions, really: (1) that one of the variables of intelligence is the ability to form and use concepts, and (2) that part, at least, of the reason why mental age increases during the period of growth is that the ability to conceptualize increases.

These assumptions seem reasonable, yet relatively few efforts have been made to verify them. What this amounts to, basically, is comparing the performance of children of high and low intelligence, estimated as best one can in situations which involve the formation and/or use of concepts. When this is done, some unexpected results may occur which force us to raise additional questions. Thus, Deutsche (**7**) found that scores on her tests of causal relations had only a low relation to intelligence test scores (Kuhlmann-Anderson). The studies of Long and Welch (**42**), as well as others, indicate that chronological age is at least as important as mental age.

Training or experience. This condition further complicates the relationship between intelligence and concept formation. Studies such as those of Deutsche (**7**), Oakes (**46**), and Ordan (**47**), suggest that children differ in their concepts and use of concepts as much because of variations in experience as in variations in intelligence. Investigations of very young children, such as those of Gellermann (**15, 16**) and Welch and Long (**73**), indicate that at a very early age children are capable of learning concepts and can, in fact, be taught them. Peterson (**48**), in a study of generalizing ability involving the principle of the lever, found that grade in school had a closer relation to performance than either age or measured intelligence. In Deutsche's investigation (**7**) the correlation between school grade and scores on her concept test (combined forms) was .445 (*PE* .067), whereas the correlation with I.Q. was only .182 (*PE* .098).

Socio-economic status. Both Deutsche (**7**) and Ordan (**47**) report that socio-economic status has only a very low relation to scores on concept tests.

Vocabulary. Both Deutsche (**7**) and Ordan (**47**) report a substantial relation between this factor and concept scores. In the former study the correlation between number of words used and scores was .48 (*PE* .06); in the latter the correlations between vocabulary and various areas[9] of social concepts were of about the same magnitude.

In summary, it may be said that the genetic aspects of concept formation have been very inadequately studied up to the present time. The most thoroughly investigated aspect of the problem appears to

[9] Crime, economics, government, war-peace, etc.

be that which relates to our Problem 2, that is, the acquisition of concepts. Problem 1, which concerns the more general question of "conceptualizing," remains comparatively untouched. Perhaps the most interesting feature of this problem is the apparently low correlation with intelligence test scores and the relatively high correlation with training and experience, of which one important component seems to be vocabulary.

Experiments with Adults

As noted previously, research at the adult level has primarily been concerned with our Problem 3, that is to say, less with concept formation in general (conceptualizing), and more with a situation in which a particular concept or group of concepts is achieved (that is, isolated from or applied to a particular set of stimuli). Although we distinguished between learning and problem-solving situations, the latter aspect is the predominant one. Although experimenters have striven to set up conditions in which the subject is confronted with new experiences out of which new concepts are supposed to emerge, it appears to the present writer unlikely that this can be done except in the case of children. Thus, with adults it seems probable that the main emphasis should be on such problems as the behavior of the subject, what concepts he uses, and how he uses them. In the discussion to follow it will be assumed that the subject is fundamentally using his past experience rather than acquiring new experience.[10] The data obtained from experiments may conveniently be considered under two principal headings, the conditions under which the material is presented, and the behavior occurring in concept formation situations.

Conditions of Presentation

The many excellent experiments in this field have contributed much to an understanding of efficiency in situations where concepts are to be used or experience is to be generalized.

Method of presenting stimuli. One of the earliest objective experiments, the famous one of Hull (**37**), was an effort to determine the factors which promote efficiency in this kind of problem-solving situation.

The method used by Hull has served as a prototype for many subsequent experiments. He prepared twelve series of stimuli each of which contained a common element or concept. These were presented successively in an exposure apparatus. With each exposure the experi-

[10] This assumption does not mean that it might not be possible to study the acquisition and conceptualizing of experience in the adult; so far, however, experiments have not convincingly done so.

menter pronounced the nonsense syllable which served as a name for the concept. In succeeding series the subject tried to anticipate the correct name of the stimulus but was corrected by the experimenter if he failed to assign the correct name to it. The ingenious stimuli used for this procedure were Chinese characters built around radicals (the common element, or concept). In this way Hull compared, for example, two orders of presenting instances of the material each of which contains a "common element." The first order is from simple-to-complex, the second from complex-to-simple. If the time spent on each instance is the same there is no apparent difference between the two orders, "but if each individual experience in the evolution series is continued until the reaction to it is just perfected before passing to the next, there is a distinct advantage in favor of the simple-to-complex method." In general, simple instances are more efficient because it takes less time to form the concept from them (i.e., extract the common element).

Context and critical features. Hull also studied the effect of emphasizing the concept (the common element) in various ways, in contrast to presenting only the total characters in which the concept was embedded. Thus, giving the common element outright is no more efficient than extracting it from a series of concrete instances. In the former situation, however, the subject can better verbalize the concept, although this is obviously not necessarily a true indication of functional value, since the subject did not show any better performance. If the common element is emphasized *in situ* by outlining it in a different color (red), it is easier to generalize it. So far as familiarity is concerned, it was found that twice as frequent exposure to half the series of instances is less efficient than moderate exposure to all the members of the series. Finally, the most efficient method of presentation was one which combined examples of the concept ("naked" common element) with concrete examples of it embedded in various contexts.

The elaborate and lengthy series of experiments performed by Heidbreder (**28** through **35**) shows other conditions of this kind. Her general procedure is similar to that of Hull; that is, it was an adaptation of the familiar memory situation in which the subject learns the nonsense syllable names for various categories of stimuli by the anticipation method. In carrying much further the principles suggested by Hull's experiments, she has found that concepts are attained more easily when the critical features (the features which identify the concept) are readily accessible perceptually and are more "thinglike" (**31, 32**). For example, concepts of objects were more quickly attained than concepts of forms, and attainment was more rapid, in general, when non-critical features occurred only once but critical features occurred repeatedly. Concepts are also attained more easily when the context provides strong perceptual support for the critical features. In general, the more thing-like the instances the more readily is the concept attained, and the more the

context supports a particular concept where several are possible, the more rapidly will the supported concept be attained. Thus, varying the context from experiment to experiment resulted in marked changes in the order of attaining concepts. (This point is further elaborated below.) Another line of evidence comes from the nature of the task. When the subject is permitted to participate perceptually more directly in the experiment, concepts are attained more readily. For example, when subjects sorted cards instead of observing the instances in the exposure apparatus, the concepts were more rapidly achieved. The use of verbal instances of the concepts instead of drawings yielded essentially similar results except that the order of attainment seemed to depend not upon the thing-character of the instance but upon "semantic efficiency."

Positive and negative instances. Kuo (**38**) used a method similar to that of Hull and found that the more negative or misleading characters there were in the series, the less accurate the conclusion. Smoke (**58**) used geometrical designs as stimuli. Each series was constructed according to a certain principle, the discovery and definition of which was the subject's task. In comparing performance on series containing both positive and negative instances of the principle with those in which only positive instances appeared, Smoke found no difference in efficiency. Actually, the situation may be more complex, for some subjects seemed to be aided by the negative examples whereas others did not profit from them. In a later study, Smoke (**59**) confirmed this result but felt obliged to qualify the conclusion to the effect that the presence of negative instances may serve as checks in the learning process and thus are of assistance, especially where difficult concepts are involved.

Set. One of the experiments by Reed (**52**) reveals the influence of set. The task of the subject was to learn the nonsense names of forty-two cards on each of which were written four common words. One word on each card belonged in one of six categories represented by a nonsense syllable. To study set, Reed instructed one group of subjects merely to learn the name of each card; a second group were to learn the name and secure the meaning and were also given information about the nature of a concept. The latter group, characterized by a specific set, showed a greatly superior performance. Reed suggests that this economy occurs because the latter group are more likely to keep the goal (learning the concepts) in mind and participate more effectively in the problem situation by searching for possibilities of grouping, testing hypotheses, etc. Ewart and Lambert (**11**) have also reported results of this kind.

Length of series. In another experiment Reed (**53**) examined the effect of varying the amount of material. Within the scope of his experiment longer series were more efficient than short series; that is, the amount of work required, though absolutely more, was relatively less for longer series. The proportion of consistent concepts achieved also increased with length of series.

Complexity of stimuli. In another experiment by Reed (**54**) the situation was the same, but the number of words on each card was increased and confusing categories were introduced. The latter, however, only applied to a few of the total possible number of instances. As might be expected, the effort required to form concepts increased directly with increased complexity. The most striking effect, meriting further research, was that the proportion of consistent or correct concepts decreased markedly with an increase in complexity, accompanied by a corresponding increase in the proportion of inconsistent or incorrect concepts. As Reed states it, "as the complexity of the stimuli is increased there is a definite trend to shift from logical to illogical learning, or to base concepts on such factors as the primacy, and frequency, and sensory similarity of contiguous stimuli."

Mutual interference. Gengerelli (**17**) studied another interesting phenomenon. He had one group evolve concepts from meaningless drawings which were named by a letter, then presented them with the same material in which the concepts were named by a different letter. A control group carried out the same procedure, except that the drawings were changed somewhat in the second stage. The experimental group showed more interference than the control group, thus indicating the difficulty of shifting the meaning of a concept from one situation to another.

From this survey of experiments it may be said that if concept formation is defined as the process of discovering an identifying detail, relation, or principle by means of which a series or collection of instances can be classified,[11] as it may be perfectly proper to do in keeping with our Problem 3, then we know to a considerable extent what will happen under various conditions of presenting the material. The major findings are as follows:

1. The order of the instances does not particularly matter if an equal amount of time is allowed to each.
2. Abstract instances alternated with concrete examples is the most efficient method.
3. Negative instances are not necessary but may be included to serve as an occasional check on accuracy.
4. The subject who is well instructed in his task will do better than the subject who is simply a passive recipient of the material.
5. Consistency is promoted if the series is long rather than short, and simple rather than complex.

[11] Dr. Leslie Briggs has, in fact, proposed to the writer this definition for concept formation: "The process through which the person manipulates and classifies the essential common features embedded in a number of complex stimulus situations in order to arrive at a symbol which represents the common elements in the total range of stimuli." This definition might well be adopted for our Problem 3.

6. A given context is likely to favor one concept over another, and the readiness of attainment will depend upon how accessible the critical features are to perception.

It is evident that much work remains to be done to ascertain how general these conclusions are. At present they must be regarded as tentative statements applicable to the kind of experimental situation upon which they are based.

Behavior in Concept Formation

Experimental studies have revealed a number of important phenomena which occur in the formation and use of concepts. To some extent, accordingly, it is possible to describe what happens when the individual is placed in a situation corresponding to the experimental conditions.

Gradual attainment. Several of the experiments which attempted to break down the successive phases of discovering the concept, mainly through the introspective method (**4, 10, 26, 37, 53, 61**), have revealed that concepts are typically evolved gradually by the subject. In general, the subject is exposed to the material and little by little begins to interpret it—perhaps by perceiving in it familiar objects or associating aspects of it with his past experience; perhaps by deliberately attempting to analyze it, comparing one instance in the experimental situation with another, etc. In time, some features begin to stand out, others to "disintegrate" (**61**), until the subject is able to formulate a principle which will work. He may, of course, fail to develop an adequate concept.

Attainment without awareness. Hull (**37**) and Smoke (**53**) both found that an individual may be able to act in a manner showing that he can act in terms of the concept without knowing what it is. That is, he can successfully identify specimens of the class without being able to verbalize the concept.

Attainment of consistent and inconsistent concepts. As previously noted, Reed (**53**) found that subjects may develop inconsistent as well as consistent concepts. In general, the development of a consistent or inconsistent concept results not from different factors but from different manifestations of the same factors. These factors are set, identical perceptual elements in the concept-symbol and the instances, frequency of occurrence of key-items belonging in a given class, contiguity between an instance and the class-symbol (e.g., position factors), and generalization. Consistent concepts are favored by a specific set directed towards group classification, whereas inconsistent concepts are more likely to occur if the set is general rather than specific. Consistent concepts are favored by the occurrence of identical elements based on meaning or group relationships, whereas inconsistent concepts are in-

fluenced by identical elements based on perceptual (and incidental) similarities.

Order of attainment. Some rather remarkable and significant behavior was observed in Heidbreder's experiments (**28** through **35**), which has been confirmed by Wenzel and Flurry (**74**). In these experiments, it was quite regularly found that concepts are formulated in a definite *order of dominance*, depending upon the nature of the material and the context. Under the conditions of these experiments the subjects first attained concepts of objects, then concepts of forms, and last, concepts of number. If, however, the stimuli are varied to make some critical features more obvious than others, there was likely to be a change in the order of dominance. Thus, in a series of twelve experiments involving card-sorting, varying conditions resulted in dominance of form, or color, or, in one case, quantity. When the form was represented by the whole card, which was also colored, and the concrete object was a small drawing on the card, the order of dominance was: color, spatial form, objects. Cards containing the forms outlined in different ways, with small objects varying in number and without color, were sorted in the order: spatial forms, objects, linear quantities, and number. On the whole, however, it was rather difficult to influence the order of dominance. In sixteen experiments where it was possible to attain concepts of objects,[12] these concepts were attained first eleven times, second four times, and last only once. Numerical quantities were attained last in ten experiments, second in two experiments, and first only once (in an experiment from which objects and forms were absent).

Concrete and abstract concepts. In experimenting with various sorting tests, Weigl (**64**) found that individuals characteristically displayed contrasting concrete and "categorical" (or more conceptual) approaches.[13] Using a variety of tests, Bolles (**2**) further analyzed these approaches, suggesting that concrete performance is primarily determined by sensory impressions whereas the more conceptual or abstract performance is a response to objects as representatives of a class or category. The "basis of pertinence" (or way in which objects are classified) was found to be of the following types:

1. *Identity*—items belong together by virtue of exact sensory equivalents.
2. *Partial identity*—similarly determined but resting upon an equivalence of only one sensory attribute.
3. *Co-functionality*—items belong together in some concrete situation.
4. *Categorical similarity*—items belong together because they represent a class, not because of similar attributes or contiguity of function.[14]

[12] Including the experiment of Wenzel and Flurry (**74**).

[13] It is interesting that English (**10**) reported substantially similar approaches.

[14] In this research, and others below, the primary emphasis was upon the comparative conceptual performance of aments and/or dements and normal persons.

Hanfmann and Kasanin (**20, 22**) developed a standardized procedure for testing conceptual performance with the Vigotsky sorting test. This test consists of a collection of small blocks, varying both in color and shape. On the bottom of each block is a nonsense syllable which serves as a clue to the proper classification of blocks into a double dichotomy (tall-large, tall-small, flat-large, and flat-small), the basis for which is not immediately obvious to the subject. After preliminary explanation of the purpose of the test the experimenter turns up one of the blocks, thus revealing its name, and asks the subject to select all those that belong with it. If the subject selects all those of the same color, for example, the experimenter turns up one of the wrongly classified blocks and asks the subject to try again, and so on until all the blocks have been correctly sorted. It is possible to score performance in terms of time taken, number of clues (turned-up blocks) required, type of solution, etc.

Through the use of this test, these investigators have distinguished three levels of performance, namely: a primitive, "concrete" performance, an intermediate performance showing some aspects of conceptual thinking but not others, and an advanced conceptual performance. For example, with regard to interpretation of the task, the subject at the primitive level does not relate the words to the properties of the blocks, with the result that the names of the blocks are disregarded and the task may be seen as solvable only by trial and error. At the intermediate level the subject does not grasp fully the nature of the required grouping; i.e., the task may be seen as a game with rules, or only one possibility of grouping is seen. At the conceptual level, the task is comprehended as involving classification. Different kinds of groups tend to be formed by individuals operating at the three levels. At the primitive level grouping may be merely random or in the form of constructions; at the intermediate level, the subject may assemble collections (such as one block of each color) or pseudo-classes (that is, all red blocks, where the basis can be shown to be "red," not color); and finally, at the conceptual level, true classes are formed.

Goldstein and Scheerer (**19**) have also used sorting tests to investigate conceptual behavior and have also found these contrasting approaches. They define the concrete attitude as realistic—that is, dependent upon immediate sensory impressions—whereas the abstract attitude is characterized by detachment from immediate reality and includes more than the "real" stimulus.

Although these investigators have been primarily concerned with pathological cases, this distinction between concrete and abstract performance may have considerable significance for normal, everyday behavior. Goldstein and Scheerer (**19**) point out that there are various degrees and gradations of these approaches. It is probable that "the normal individual combines both attitudes and is able to shift from one

to the other at will according to the demands of the situation." More work with normal subjects is clearly needed in this area, since other experiments with different emphasis have strongly suggested that these attitudes are important aspects of concept formation and use. Hanfmann (**21**), in fact, has presented preliminary evidence based on the Vigotsky test for "personal patterns" of approach in tasks involving concepts. In the predominantly "thinking" or conceptual (abstract) approach, the subject develops hypotheses without paying much attention to the blocks, which are utilized to check the hypotheses; all of this may be accomplished without actually touching the blocks. On the other hand, in the predominantly "perceptual" (concrete) approach, the individual keeps in close touch with the material and gets his ideas from looking at or handling the blocks. A predominant approach of one kind or the other is characterized as a "concordant" pattern, whereas in a "discordant" pattern the two tendencies conflict with each other, leading to a less efficient performance.

Concluding Statement

In this review of concept formation we have been concerned to show that it will ultimately be necessary to take into account a wide variety of facts before an adequate formulation can be achieved. In general, concepts represent regulating (or selective) systems in the mental organization of the individual—systems which link separate sensory impressions, which depend upon past experience, and which are organized apart from direct sensory data. A given concept has both extensional and intensional meanings; it may have a consistent or inconsistent organization; it has important hierarchical relationships to other concepts. Furthermore, an adequate distinction must be made between the selective mechanism of the concept and the response selected—whether verbal (such as naming), manual (such as reaching for an object), or covert (such as an image).

So far as the available experimental data go, it appears that three major problems have been studied but without sufficiently clear differentiation between them. We have suggested that one problem is concerned with the development of the ability to conceptualize (to form and use concepts), another with the acquisition of a repertory of concepts (building up an organization of particular concepts), and still another with how any particular concept is achieved. The first two problems are basically genetic problems which must be approached primarily through tracing the learning process in the child; the third problem is, of course, important in child behavior, but it has been studied primarily at the adult level. Indeed, we have pointed out that it seems to be the only problem of concept formation which can be investigated

at the adult level inasmuch as the adult has already developed an ability to conceptualize and already possesses so great a store of concepts that "new" ones are probably applications or reorganizations of previously learned ones. Nevertheless, it is still important to find out more than we now know about the characteristics of adult concepts, about how the repertory is used in a perceptual or learning or problem-solving situation.

Despite the magnitude of the problems, only a comparatively few methods have been used to investigate them. With children, the two methods have been an interview-questionnaire technique and a performance situation. Although the two genetic problems have not clearly been distinguished, as pointed out above, the latter method seems to have been applied mainly to Problem 1 (that is, the performance of the child is tested to see if he can conceptualize in various situations), and the former method to Problem 2 (that is, the child is asked questions in order to determine what concepts he has, how he uses them, etc.) With adults, the methodology has included introspection, learning situations, and problem-solving. The favorite procedure in the learning situation has been the anticipation method. Most experiments, however, have not included a preliminary learning period but have resembled problem-situations.

Bearing in mind the limitations imposed by these methods—for example, that the intensional and inconsistent aspects have been largely ignored—we can, nevertheless, note that a certain amount of progress has been made towards answering the three currently identifiable problems in the investigation of concept formation.

Problem 1, which concerns conceptualizing ability in general, has been the most inadequately investigated of the three. We do not really know for certain that there is an ability to conceptualize which might be distinguished from other perceptual and intellectual functions. Undoubtedly, children's concepts change with age, but whether or not this reflects development in a general function to form and use concepts has not been convincingly demonstrated. Several studies suggest that children can and do form concepts at a very early age and that the chief differences in this function between children and adults are of degree rather than kind. Thus, children have fewer concepts, perhaps because they have not yet had sufficient experience rather than because they do not yet have sufficient conceptualizing ability. Similarly, they cannot verbalize their concepts as well as can adults, but this does not necessarily mean that they do not have the concepts. An important finding in this connection, which warrants much more research, is that "concept scores" determined in various ways have a low correlation with intelligence test scores but a high correlation with training and experience.

With regard to Problem 2 (the acquisition of a repertory of concepts) much more is known. In fact, most investigations of children have dealt primarily

with it. Here it has been found that concepts tend to change with increasing age, becoming more numerous, more complex, and more "logical." It has been tentatively ascertained that children acquire concepts in a definite order, although more systematic studies at very early ages are needed. For example, triangularity can be established as early as fifteen months, roundness by the age of three years, and size relations somewhat later. Concepts of time, social concepts, etc., are acquired later and undergo long development up into adulthood.

Finally, Problem 3 (the formation of a particular concept) has been dealt with more intensively than either of the other two. The great bulk of the research with adults has been concerned with discovering what happens when an individual evolves a particular concept or group of concepts out of a set of stimulus material. It has been found that performance of this kind is influenced by many conditions of presenting the stimuli, such as: (a) different ways of presenting the stimuli (order of presentation, varying the complexity; (b) contextual factors; (c) the inclusion of negative as well as positive instances; (d) set; (e) interference between similar series. In addition to these results bearing upon objective features of the concept-forming situation, what might be called "concept behavior" has been extensively observed. Among the characteristics noted are the tendency for subjects to evolve concepts gradually, the occasional attainment of concepts without awareness of the concept itself, the development of inconsistent as well as consistent concepts, the tendency for concepts to be learned in a definite order, and the occurrence of varying degrees of concrete and abstract approaches to, and use of, concepts.

Out of all these findings at least four significant points emerge for future investigators to consider.

1. The first is the distinction often mentioned above between child and adult concept-formation. The development of conceptualizing as a general function merits especial attention.

2. The second point is the surprising paucity of methods which have been employed to investigate the problems of concept-formation. The methodology, therefore, should be extended and elaborated.

3. The third point is the need to recognize more clearly than has been the case heretofore that concepts have internal and organizational characteristics as well as those objectively definable in terms of the external (e.g., verbal) behavior of the individual.

4. Finally, most investigators have concerned themselves only with very limited aspects of concept formation, so that research on a wider scale is needed along with conceptual integration of all the varied facts discussed in this article.

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METHODOLOGY IN PROPAGANDA RESEARCH[1]

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Two major topics are taken up in this review. The first of these relates to the unsettled systematic status of propaganda, including the variety of ways in which propaganda phenomena have been conceptualized. In the course of developing this topic the framework for a methodological analysis of propaganda will be presented and illustrated. The second major topic concerns some of the more important efforts to study propaganda under experimental conditions and calls attention to some of the difficulties of such study.

The Unsettled Systematic Status of Propaganda

There is ample room for efforts to organize the facts and to theorize about propaganda, because no matter what one writes he can be fairly sure of not running counter to an experimentally verified and clearly organized body of fact and theory. A psychologist offering a neural perservation theory of "reminiscence" would promptly be shown to be at variance with established fact and theory. The thinker about propaganda, on the other hand, is almost free to conceptualize as he pleases without threat of serious professional consequences.

This section of the paper will illustrate several efforts to establish viewpoints concerning propaganda. In so doing it will illustrate the fact that a widely accepted systematic outlook in propaganda is lacking. Conceptualizations by different writers will be reviewed and several definitions of propaganda presented and discussed.

First, however, a note on the meaning of "conceptualization" as used in this paper will establish the framework of the following discussion. A *conceptualization of propaganda* is the specification of the independent, intervening (or intermediary), and dependent variables involved in propagandizing, together with statements or propositions showing how variables of the three classes relate to one another.

For purposes of the present analysis, the class of independent variables will include the doings of propagandists and the resulting propaganda stimuli which impinge on the propagandee.

The class of intermediary variables[2] includes (a) measures or indices

[1] The writer is indebted to Dr. Floyd H. Allport for certain trends of thought underlying this paper and for advice on certain aspects of the presentation.

[2] The term "intermediary variable" is suggested in place of the older "intervening variable" as used by Hull, Tolman, and other learning theorists. There are two reasons

of the social structuring of propagandees (*societal* intermediary variables) and (b) measures of the propagandees' attitudes, values, teleonomic trends, traits, needs, or other appropriate psychological constructs (*psychological* intermediary variables). Like intervening variables and hypothetical constructs in learning theory, variables in this class are thought of as bridging the gap between stimulus variables and response variables, explaining the nature of the relationship between them.

The class of dependent variables here includes measures of the propagandees' responses to propaganda—change in attitude, memory for propaganda content, overt actions of specified kinds, etc.

It should be pointed out that the two sub-classes of intermediary variables, societal and psychological, while having the common function of explaining the connection between the stimuli presented by the propagandist and the responses of the propagandee, are not assumed to be on the same level of logical discourse. *Psychological* intermediary variables are in the nature of hypothetical constructs or intervening variables (cf. MacCorquodale and Meehl's discussion of these terms [30]) and as such are inferred from concrete data; they do not themselves generate the data. *Societal* intermediary variables, however, may be observed directly. That is, the data of the variable may describe, or otherwise refer directly to, objective facts within society. For example, one could compute an index based on knowledge of a person's socioeconomic status, religious affiliation, and other special group memberships, which might account for the differences between his and others' responses to common propaganda stimulus variables. Such an index, and the conditions it reflects, would not be "caused" or otherwise influenced by the propagandist (or the experimenter) and so would not be an independent variable as that term is used in the present analysis. (The index could, of course, be "manipulated" statistically once obtained, just as indices of psychological intermediary variables could.)

Likewise, the social structures mediating propaganda effects may also be considered as societal intermediary variables. For example, people may be urged to put pressure on their legislators in support of a propagandist's objective. The final "putting of pressure" depends on the existence and availability to the propagandees of the specialized social structures mediating communication (mails, etc.). Thus the un-

for this. First, data about certain variables in this class (e.g., indices of social structuring) are obtainable apart from the organism whose behavior is in question, whereas the intervening variables of the learning theorist are presumed to represent conditions or dispositions *within* the organism. Second, intermediary variables in the two major sub-classes are defined and measured by distinctly different procedures and are different in logical structure, as the following discussion brings out.

derstanding of this particular dependent variable ("number of urgings delivered to legislator X") hinges to some extent on a societal intermediary variable that could be indexed as "availability of communication channels to propagandees."

The choice of specific intermediary variables in either sub-class will, of course, be influenced by the particular dependent variable the experimenter wishes to examine, as well as by his particular theoretical preferences.[3]

The several conceptualizations discovered for discussion in this paper seem to fall into three fairly distinct categories. These categories may be briefly characterized before taking them up separately in detail:

1. *Linear conceptualizations* tend to suggest that propagandas operate as forces, each trying to push its recipients in some particular direction. In such conceptualizations emphasis is mainly on such independent variables as the content itself or the medium of its delivery, with relatively little attention being given to intermediary variables such as the social structuring of the propagandees and their psychological characteristics. Seldom, too, is effort given to specifying systematically the nature of the outcome, or dependent variable.

2. *Psychological conceptualizations* emphasize psychological intermediary variables such as the attitude and value systems of the propagandee and give little attention to the content of the propaganda or the propagandees' social structuring. Dependent variables are most commonly construed as psychological data: attitudes, opinions, beliefs, etc.

3. *Situational conceptualizations*[4] stress intermediary variables comprising the objective social structures in which propaganda must operate. Other independent variables and psychological intermediary variables are given less attention. Dependent variables are commonly construed as some form of social action as well as psychological data.

4. Occasionally a writer whose offerings are best placed in the situational category edges over into a fourth class of conceptualization which may be called *structural*. Here the effort is made to imagine all the possible variables that could be related to the effectiveness of a propaganda effort. The meaning of this as yet practically unoccupied category will be developed later.

Quite obviously, the discussions in the sources to be cited are not arranged so as to permit unambiguous identification of the variables the writers had in mind. But in organizing the available materials the present writer has tried to ferret out the others' meanings and to consider them in the framework just described, in order to compare them more

[3] Relevant to our discussion of societal intermediary variables, an incidental topic in an earlier paper by Tolman (41) points somewhat in the same direction.

[4] The term "situational" is used in preference to "social," "societal," or "cultural" because of the intention to refer to the social situation (or social structuring) of the propagandee, rather than to the broader conditions characterizing the total culture in which the propaganda occurs.

easily and to evaluate them more fairly. Terms identifying the variables apparently had in mind by the various writers are bracketed within quotations.

Linear[4a] Conceptualizations

The clearest example of a linear view of propaganda is that of Moss (35), who says:

> Propaganda is an indirect type of appeal through suggestion [independent variable], which makes possible the management [dependent variable] of people. Propaganda may sell [dependent variable] a commodity, an idea, or a man—not on the basis of merit but through emotional and suggestive appeals [independent variable] which strike a sympathetic chord in the imagination [figure of speech?] of the public. . . . So powerful is political propaganda when skillfully manipulated [independent variable] that the mass of voters can be made to believe [dependent variable] one thing today, a quite different thing next week, and its opposite a month hence (pp. 424–425).

Here propaganda is a force, presumably taking the form of words, that may be turned on or off according to the desires of the person controlling it.

Another linear conception of propaganda is portrayed in *The Fine Art of Propaganda* (46), published under the auspices of the Institute for Propaganda Analysis. Here propaganda is defined as "opinion expressed for the purpose [independent variable] of influencing reaction [dependent variable] of individuals and groups," and the bulk of the book is devoted to illustrating in Father Coughlin's speeches "The Tricks of the Trade," the now well known seven "devices" that propagandists are said to apply in influencing the action of others. The "devices" are treated as tools that may be used at the whim of the propagandist, and it is pointed out that "Once we know these devices well enough to spot examples of their use, we have taken a great and long step toward freeing our minds from control by propagandists" (p. 24). Throughout the book no reference is made to what effect personal characteristics or social structuring of the victims of the devices may have on reactions to propaganda. The emphasis is entirely on the forces inherent in the "Tricks of the Trade."

Efforts at content analysis, exemplified in some of the work of Lazarsfeld and Merton (26) stem from an essentially linear conception

[4a] The concept of the "linear" approach in psychological methodology was formulated by Floyd H. Allport (unpublished work) as a means of distinguishing the more customary logic from a "structural approach" upon which he is now working. The application of the concept, made here to propaganda studies, including the dependent-independent variable logic as an earmark of linearity, was developed by the present writer.

of propaganda, though other works by these two writers fall well into other categories. Lazarsfeld and Merton define propaganda as "any and all sets of symbols [independent variable] which influence opinion, belief, or action [dependent variable] on issues regarded by the community as controversial" (**26**, p. 58). They then report studies in which definite features of propaganda—that is, features of the symbols—are linked with definite types of response. In their procedures, the symbols making up the propaganda are analyzed for particular themes, classified as favorable or unfavorable with respect to some propaganda objective; or the interrelationships of themes are studied. Such characteristics of propaganda are then correlated with "response analysis" data obtained through interview. Though a great emphasis on this type of approach to propaganda could lead to de-emphasis of social and psychological characteristics of the propagandees, Lazarsfeld and Merton recognize the limitations of this approach, and incidentally add a *situational* meaning to their conception of propaganda, in pointing out that "no propaganda [independent variable] can prevail [dependent variable] if it runs counter to events and forces [societal-psychological (?) intermediary variable] underlying those events. . . . Propaganda is no substitute for social policy and social action . . . " (p. 79).

Another point of view on propaganda illustrating mixture of two categories, this time linear and psychological, is that of Lindesmith and Strauss (**29**). These writers say that propagandizing "refers to the attempted [independent variable] conversion [dependent variable] of masses of men and women to a point of view. Successful propaganda [independent variable] ends in mass results [dependent variable]: it causes numbers of people to act together as a group, or at best to act in similar ways. It may also be used to undermine the morale of opponents" (pp. 499–500). They then go on to list "techniques" the propagandist may use to get his results. However, in the discussion of the limitations of propaganda, their conceptualization appears to fall on the borderline between the linear and psychological categories: "The propagandist must play [independent variable] on the response system of people [psychological intermediary variable] and sometimes he will find his human subjects very recalcitrant [dependent variable]" (p. 503); this idea is then elaborated somewhat. Still less recognition is given to situational factors as a limitation on the propagandist when Lindesmith and Strauss point out that he may run against the propagandees' conceptions of themselves as fathers, churchgoers, and loyal citizens if he attacks basic institutions such as the family, church, or government. But this admixture is still largely psychological in that it deals with various concepts of self that may arise from institutional role-playing.

Strengths and weaknesses peculiar to linear conceptualizations. Here the major strong point seems to be the development of methods of analyzing propaganda content and of ways of recognizing how various utterances in public communication channels are slanted toward certain viewpoints. Curiously, though, this advantage grows out of the principal weakness of linear approaches, namely the overemphasis of one kind of independent variable—the verbal output of propagandists. More work could well be done with content analysis, but it might be more fruitful to give additional effort to identifying the particular psychological intermediary variables to which the various categories of content might be relevant. Efforts in this direction could well take "response analysis" (26) procedures as a point of departure.

There are other weaknesses in linear conceptualizations common to other conceptualizations which, to avoid repetition, are taken up after the others are considered (see pp. 43–45).

Psychological Conceptualizations

Psychological conceptualizations of propaganda are those more clearly emphasizing the attitudes, values, and other personal characteristics of propagandees (both as psychological intermediary variables and as dependent variables). The most clear-cut example of a purely psychological approach to propaganda is that of Krech and Crutchfield (23). In its very nature phenomenological, the psychology of Krech and Crutchfield cannot give emphasis to social structurings or to the symbol-content of propaganda apart from the way those things relate to the individual's perceptions as determined by his needs and values. Response to any stimulus, propaganda content included, depends solely on the cognitive reorganization of the "psychological field" of the individual at the time of stimulation. The psychological field, they say, is "a cross section, at a given moment, of the psychological existence of the individual. It includes only what is present at the moment, not what was present previously or what may be present in the future" (p. 33). Elsewhere, Krech and Crutchfield identify the psychological field as "a state of neuropsychic processes that are reflected in the form of the *experienced* world of the individual and in the form of patterns of ***neural processes in the brain*** of the individual. The psychological field is not identical with the experiences of the individual for there are important constituents of the neuropsychic process that are unconscious, i.e., do not result in immediate experiences by the individual" (p. 37). In their treatment of propaganda, then, these writers look upon words as objects in the real world, objects as real as the things for which they stand, and the propagandist thus appears as a man who can alter other people's worlds by introducing words into their environments just as he might by introducing people, machines, or furniture. "Propaganda analysis" for them would be mainly "concerned with the current beliefs, needs,

percepts, attitudes of the listener" (p. 329). In a very subjective sense, then, their approach is purely psychological, dealing with the private experiences of the person. No effort is made to relate propaganda efforts and effects to objective collective relationships of the individual, for these things may or may not be important, depending on the perception of them by the person. Krech and Crutchfield are not clear about how the psychological field of a person at a given instant is to be ascertained for purposes of prediction, but that is not a problem for this paper.

Another viewpoint predominantly psychological, although having noticeable strains of *linearity*, is that of Doob (**15**). Doob defines propaganda as "the attempt [independent variable] to affect the personalities [psychological intermediary variable?] and to control the behavior of individuals toward ends [dependent variable] considered unscientific or of doubtful value [psychological intermediary variable] in a society [societal intermediary variable] at a particular time" (p. 240). The last seven words of the definition convey the only implication in Doob's treatment about the importance of social organization in the study of propaganda. He develops this phrase only in an effort to distinguish, as many writers do, education from propaganda. But beyond this differentiation, Doob's treatment of propaganda is almost strictly psychological, as suggested by the titles of his chapters 14, 15 and 16: "The Perception of Propaganda," "Personality and Propaganda," "The Learning of Propaganda." Chapter 13, "The Content of Propaganda," suggests linearity of approach, for here we find methods for determining and adjusting the relative proportion of various appeals and themes from which the unit of propaganda derives its strength.

To illustrate further Doob's psychological viewpoint we consider some of the details of the chapters just mentioned. In chapter 14, Doob speaks of auxiliary responses, roughly equivalent to attitudes, whose function is to produce the perception at the basis of the learning of propaganda. The propagandee's action results from "propaganda he has heard and from other attitudes within him. The responses facilitating perception have been merely auxiliary though absolutely necessary for the final behavior" (p. 321). Also in this chapter Doob takes up such standard topics in perception as stimulus intensity, stimulus context, stimulus simplification, perceptual repetition, and perceptual variation, and illustrates how these factors operate in various forms of propaganda from advertising to psychological warfare. In chapter 15 Doob emphasizes personality characteristics that the propagandist finds related to the propaganda objectives (that is, a "personality response" which is "instrumental in achieving a goal response or which

mediates such a response. . . . In the case of propaganda the goal response to which a personality response is related or unrelated is the propaganda objective"). Other related material comes in chapter 16 where Doob mentions three reasons why personality responses aroused by propaganda may not result in the recipient's learning a new attitude. These are (1) their drive strength may be weak, (2) the responses may be unrelated to the propaganda goal, and (3) other personality responses previously aroused in connection with the propaganda goal may have greater drive strength.

Further research may show that an idea suggested in the material just cited represents a real advance in propaganda theory, viz., the relationship of personality characteristics to *action in bringing about* the propaganda objective. The role of personality in this context has received little attention, more study having been devoted to characteristics that may be associated with the propagandee's *becoming convinced of the desirability of the objective itself.*

A. M. Lee (**27**) mentions, among others (see below, pp. 41–42), a psychological approach which expresses the idea that social struggles and their propagandas attract personality types that can find needed expression in the resulting appeals, promises, and actions, and that such personalities once attracted have strengths and limitations that are felt throughout the entire conflict. Though Lee advances no evidence for this view, it is an interesting suggestion, similar to the one mentioned in the preceding paragraph, and one that should be specifically tested in an experiment.

Another largely psychological conception of propaganda, though this time with situational leanings, is given in the recent compendium of Smith, Lasswell, and Casey (**40**). Their statement that the "content and control of propaganda are directly related to social structure" (p. 2) suggests a situational type of orientation. But their definition of social structure as "the basic values of a given society and the pattern followed in distributing them" (p. 2)—together with the observation that "limits are put on the public media by the attitudinal standards of the community as a whole, which include morality, loyalties, expectations" (p. 2)—reveal a strongly psychological orientation. Although some recognition is given to collective involvements of the members of the public, these involvements are seen as important only insofar as they produce attitudes and values in people. The relative emphasis of collective vs. individual determinants of response to propaganda leads us to classify the approach as psychological.

Bartlett's (**6**) view is likewise psychologically oriented. He defines

propaganda as "an attempt [independent variable] to influence opinion and conduct—in such a manner [independent variable] that the persons who adopt [dependent variable] the opinions and behavior indicated do so without themselves making any definite search [psychological intermediary variable] for reasons" (pp. 5–6). The essential process in propaganda is, for Bartlett, suggestion—largely prestige suggestion—which releases certain tendencies [psychological intermediary variable] already set to go. Bartlett does not discuss social or group allegiances of people as importantly related to responses to propaganda, and he attempts a psychological distinction between propaganda and education. The latter he sees also as attempts to influence thinking and conduct, but in education the individual is encouraged to seek to understand for himself why he does what he does.

Another psychological approach is that of Bird (**8**), who defines propaganda as "planned and systematically applied suggestion, expressed largely in symbols [independent variable], for the purpose of controlling attitudes and securing a predetermined mode of conduct [dependent variable]" (p. 310). Bird states further that "Propaganda is above all else addressed to and is successful through feelings and emotions [psychological intermediary variable]" (p. 313). Other predominantly psychological views are those of Kimball Young (**45**), A. L. Edwards (**19**), and E. L. Bernays (**7**).

Strength and weaknesses peculiar to psychological conceptualizations. The advantage in psychological approaches to propaganda lies obviously in the search for important psychological intermediary variables to tie together propaganda stimulus and propagandee response. After all, it may be argued, it is an organism whose response is the center of interest in propaganda as in other fields of behavior, and the more we can know about the psychological attributes of that organism the higher will be the accuracy with which we predict its behavior. This, essentially, seems to be the view of phenomenologists, who, in theory, frankly discount the importance of conditions not a part of the organism's psychological field or life-space.

As noted above, Doob has suggested the utility of studying personality characteristics that might predispose the propagandee *to do things toward* a propaganda objective in addition to those predisposing to *believing in the merits of* a propaganda objective; this is a type of psychological intermediary variable not suggested by other writers. Further research may show this to be a positive value in the study of psychological variables in propaganda.

The concentration upon psychological intermediary variables in psychological conceptualizations means that societal intermediary variables tend to be overlooked. While it may be taken as a sort of programmatic assumption that most psychological characteristics arise from experiences in social structures, social psychology still lacks specific and detailed knowledge of the relationship between the objective facts of social structures and various psychological

phenomena. This means that we cannot justify high confidence in predictions of propaganda effects (and other behaviors) in the absence of measures and indices of relevant social structures made apart from, and considered in addition to, psychological variables. A second weakness in typical psychological conceptualizations (Doob's being an exception) is the relative lack of concern with independent variables per se; although in the over-all study of propaganda these variables are strongly emphasized, the relationship between them and psychological intermediary variables deserves more study.

Situational Conceptualizations

Situational conceptualizations of propaganda give relatively greater consideration than the two preceding types of treatment to the importance of collective organizations in which the propaganda recipients are involved. It should be re-emphasized that the categories around which the present discussion is organized are not mutually exclusive but differ from one another mainly in degree of emphasis on the various classes of variables in terms of which propaganda phenomena may be analyzed and upon which the prediction of the propagandee's response may be based. The present category includes views with considerable psychological emphasis, but they appear to stress social structures relatively more than those given earlier.

Illustrative of the situational viewpoint is that of LaPiere and Farnsworth (**24**), who point out that the direct effects of propaganda have often been exaggerated. They advance the view that it is "to the social conditions [societal intermediary variable] which make people susceptible to conversion [dependent variable], rather than to the propaganda itself [independent variable], that we must first look for an explanation of political or other leadership which has used propaganda to secure leadership status" (p. 447). While recognizing the importance of the psychology of the propagandees ("If they are reasonably content with what they have, propaganda will not be very effective," p. 447) they also recognize, by implication, that the peculiar individual "psychologies" arise from the social structurings of the time and place.

Another situational approach is followed by A. M. Lee (**27**). He first defines propaganda as "the use of words, symbols, ideas, events, and personalities with the intention [independent variable] of forwarding or attacking [dependent variable] an interest, cause, project, institution, or person [societal intermediary or dependent?] in the eyes and minds of a public [dependent or societal intermediary?]" (p. 127). After this rather loose definition, Lee describes a number of approaches to propaganda, the first of which is the "societal approach." Here he makes the point that propaganda arises and participates in social tensions and

struggles and that environmental conditions set limits to societal change which in turn control the effectiveness that propaganda may have. Lee says that the investigator or propagandist should study (a) the nature of underlying social tensions and struggles and (b) who is competing with whom and why.

Other situational approaches are the following: Parsons (**37**) defines propaganda as one kind of attempt [independent variable] to influence attitudes and hence the actions of people [dependent variable] by linguistic stimuli [independent variable], and states that propaganda [independent variable] if it is to be successful [dependent variable] must be developed in relation to both the individual [psychological intermediary variable?] and the social institutions [societal intermediary variable] in which it is to work. R. H. Turner (**42**) also says that propaganda effectiveness [dependent variable] depends on techniques [independent variable] and the receptiveness of the social situation [societal intermediary variable] and interaction with the particular social situation [societal intermediary variable].

The clearest attempt reported thus far actually to study propaganda in a real life social situation is that of Lazarsfeld, Berelson, and Gaudet (**25**). Although this report contains scarcely any direct discussion of approaches to propaganda per se, the methods and data that are presented reveal the situational approach in its clearest form. This study of "how the voter makes up his mind in a presidential campaign" describes, in terms as objective as the available methods permit, the institutional allegiances, economic status, and family loyalties which were related to voting behavior and the effects of the propaganda campaign upon such behavior. In other words, the study carefully analyzed the social situations in which a propaganda war was being waged, and sought, with considerable success, to identify those components of the situation related to several propaganda effects. The methods and some of the results of the study are reported in more detail in the next section.

Strengths and weaknesses peculiar to situational conceptualizations. To this writer there seem to be two main strengths in situational conceptualizations. The first of these is in the concentration on social structuring (or societal intermediary variables), which is not only the scene of propaganda activities but constitutes some of the major conditions which produce the frustrations and anticipations of frustrations giving rise to propaganda in the first place. It seems likely that no full understanding of propaganda can be had without recourse to knowledge of social structures likely to be influenced by the attainment of propaganda objectives and of the roles of propagandees in those structures. This leads to the second apparent strength in situational conceptualizations: a tendency toward the recognition that the dependent variable in a propaganda campaign is eventually to be reflected in some realignment of social

forces or objective change in some aspect (however small or great) of the social structure.[5] This being so, dependent variables are to be predicted, in part at least, from indices based on what are here called societal intermediary variables. The use by Lazarsfeld, Berelson, and Gaudet (25) of an "Index of Political Predisposition" is an example of this.

Among weaknesses in situational conceptualizations may be noted a lack of concern with independent variables—content and the doings of the propagandist—and relative lack of systematic concern with psychological intermediary variables (understandable in consideration of the training and interests of most writers on propaganda whose products would be classed as situational). Also, much work remains to be done on how to assess for the propagandee his particular involvements or roles in various structures of society in order to relate clearly these involvements and roles to his response to propaganda (a version of one of the perennial problems of social psychology).

Summary of Methodological Weaknesses Common to Conceptualizations Considered Thus Far

The following paragraphs have a generally critical tone, so let it first be emphasized that the conceptualizations reviewed above when taken together contain much that is good and useful, though it is not yet available in systematic language and organization. This section emphasizes common methodological weaknesses in the hope that past and present shortcomings may be avoided in a more positive contribution.

1. First, and most general, definitions and discussions of propaganda tend to be quite loose and "global," often failing to recognize the need to distinguish the three major classes of variables which must be specified before research can proceed systematically. For example, when Lee (27) defines propaganda as "the use of words, symbols, ideas, events, and personalities with the intention of forwarding or attacking an interest, cause, project, institution, or person in the eyes and minds of a public" it is difficult to know what variables are meant to be indicated by these phrases. Should the independent variable be "use of" or "use of words . . . and personalities," or should "intention" be included? Perhaps "intention" would be a part of a societal intermediary variable, since it implies (with " . . . of forwarding or attacking a cause. . . . ") a context of social conflict. Perhaps "forwarding or attacking . . . " should be thought of as dependent variables since they may be outcomes of "use of. . . . " At any rate, unless a definition—accompanied by appropriate clarifying statements—really *defines* and *delimits*, it is of little use as a starting point for scientific research and understanding.

2. The treatment of independent variables (assuming they are clearly identified) is a more specific trouble area. As mentioned, content analysis is a well developed aspect of propaganda research. But other independent variable

[5] A comparison of definitions in this and the section on psychological conceptualizations will show greater emphasis in the latter on "belief, attitude, and opinion," whereas definitions in the present section more clearly imply overt action of one kind or another.

attributes should be explored more carefully: source, frequency, mode and context of content presentation; saturation (percentage of a given communication medium occupied by a propaganda); clarity, and the directness-indirectness dimension; specificity of action urged. All of these terms (and probably others could be found) represent independent variables which could be systematically experimented with for years provided one had a good conceptual system to work with.

3. The treatment of intermediary variables is a third trouble area. Sociologists and psychologists now have available a number of indices (see Appendix to 25 for several interesting examples) and tests which may be used to develop intermediary variables of both the societal and psychological types for use in propaganda and other social psychological experiments. Most propaganda experiments at all concerned with intermediary variables have used only one of the two subtypes discussed in this paper (cf. 20, 33, 39). The studies by Lazarsfeld, Berelson, and Gaudet (25) and Merton (34), and an as yet unpublished experiment by the writer, have made some effort to understand propaganda campaign effects through the use of both kinds of intermediary variables. So long as both types have predictive value independently of one another, efforts should be made to integrate the two and to explore their interrelations more fully.

The phenomenological approach (cf. 23), incidentally, seems to this writer to contain a risk of certain methodological confusions of psychological intermediary with dependent variables. If one emphasizes the importance of how-the-propagandee-perceives-the-propaganda as a determinant of his reaction to it, he must not overlook the fact that perceptions are known to the outer world only by some response. Strictly speaking, this informing response (informing about the perception of the propaganda—not about the perception of other, possibly predictively related conditions) is a dependent variable and cannot legitimately be used to infer some previous construct from which it is itself to be predicted or explained ex post facto. The predictor must be observed or inferred independently of the predicted.

4. The treatment of dependent variables is a fourth trouble area needing careful systematization. A review of the several definitions of propaganda given on previous pages will show that such terms as *opinion*, *sell*, *attitude*, *action*, *reaction*, *conversion*, and *undermine* are used to point to outcomes of propagandizing. Little effort is given to specifying how such terms may be made operational (i.e., their referents measured)—viz., how a "reaction" is to be identified, how a public is to be recognized as "sold" on some idea, or just what constitutes a "conversion." Lazarsfeld, Berelson, and Gaudet (25) may again be cited as having been wide awake to the various outcomes which a propaganda campaign may have.

In the last analysis, the success of a propaganda campaign is to be revealed in some form of social action, and the researcher must be ready to specify how the particular dependent variables (e.g., attitudes, beliefs, etc.) he is studying in his particular situation are related to action—this is a region for some very profitable basic research. Doob's (15, chap. 4) analysis of auxiliary response-perception-learning-action appears definitely as a step in the right methodological direction. The whole problem of relating attitudes, beliefs, needs, opinions, etc. to overt action is of course a central one in psychology; its solution is being

actively sought by researchers of several theoretical persuasions and would materially advance our understanding not only of propaganda but of many other problems as well.

5. The main evidence that the study of propaganda is still at a rather empirical or common sense level is in the absence of statements or propositions showing how variables of the different classes are related to one another. Linear conceptualizations are highly empirical, sometimes overlooking altogether not only the problem of relationships between variables but the very existence of intermediary variables. What is needed, for example, is a clear notion of how and why a given mass of propaganda stimuli (independent variables) are perceived in the first place: (a) what personality characteristics, values, attitudes, drives, etc. (psychological intermediary variables) contribute to perception or failure of perception, (b) how a given perception gives rise to a measurable attitudinal response (dependent variable), (c) how attitude (or belief or opinion) is related to other behaviors (dependent variable), and (d) how involvement (membership, role-playing) in a given set of groups and institutions (societal intermediary variables) is translated into psychological variables and thence into response. Psychology is far from barren of resources, both technical and theoretical, for handling such problems (e.g., see **15, 23**), and it is not the present writer's intent to imply that no work has yet been seriously undertaken toward solving them. Rather it is his hope that more actual experimentation in genuine propaganda situations—in addition to pilot studies of a laboratory sort (such as **28**)—might be undertaken to examine the significance of different variables and the relationships among them.

The Need for a Structural Conceptualization

In general terms, we mean by a "structural conceptualization of propaganda" the following: a set of definitions and propositions which endeavors to give adequate consideration to variables of all classes, and to encompass advantages already noted in the linear, psychological, and situational conceptualizations related to the persuading of large numbers of people toward some objective. This view stresses especially the relationships among those variables—how they are *structured together* to produce a propaganda campaign.

A rather unsystematic and yet broad view of propaganda is presented in a recent essay by Flowerman (**18**) which comes fairly close to the notion of a structural approach. Although Flowerman's article deals with propaganda in the "war against bigotry," what he has to say may well be considered in application to propaganda in general. Flowerman takes up three general topics, each with a number of subtopics: (1) conditions necessary for mass propaganda to be effective, (2) content of propaganda, (3) effectiveness of propaganda.

1. Under the first topic he lists *control* by the propagandist of the channels of mass communication, *saturation* (referring to the proportion of the total mass of communication made up by the particular propaganda under considera-

tion), *focussing of attention* (influenced by mode, source, context and frequency of symbol presentation), *comprehension* of the propaganda by the receivers, and *conformity* of the propaganda to group standards (i.e., standards of groups with which propagandee identifies). These factors clearly include conditions not made at all explicit in previously cited treatments of propaganda; they include factors both within the individual (e.g., comprehension), outside him (e.g., saturation), and his particular social relationships (e.g., conformity of propaganda to group standards).

2. Flowerman's discussion of propaganda content is brief and not particularly enlightening as compared with what other writers, notably Doob (15) and Lazarsfeld and Merton (26), have given us. He does point out a need for clear-cut, unambiguous presentation of the propaganda objectives and cites the experiment of Cooper and Jahoda (12) and an unpublished study by Berelson as evidence that people unsympathetic with the objective of the propaganda may misinterpret or fail completely to understand the propaganda if it is vague or ambiguous.

3. Flowerman's discussion of ways of evaluating the effectiveness of propaganda includes criteria implied in many definitions of propaganda: voting, writing letters to legislators, persuading others, writing in for more propaganda. He also mentions another measure implied by many writers and used in many experiments related to propaganda: scales for the measurement of attitudes and information. But in addition Flowerman suggests immediate tests of recall of the propaganda, delayed recall tests, and repetition of the propaganda to others.

Flowerman's statement, while containing no effort at systematic analysis into variables of different classes, makes explicit a number of things clearly relevant to a going propaganda program which are mentioned only piecemeal here and there by other writers. His treatment includes many items which should go into what we are calling a *structural* conceptualization, whose major requirements may be stated as follows:

1. A structural conceptualization of propaganda would consider the effects which the attainment of the propaganda objective would have on groups and institutions important to the propagandee; that is, *it would relate dependent and societal intermediary variables.*

2. It would study personality characteristics of the propagandee both as they relate to (a) acceptance of the propagandist's arguments and (b) taking available actions toward bringing about the propaganda objective; that is, *it would relate dependent and psychological intermediary variables.*

3. A structural approach would also include a study of propaganda content, as the appeals used relate to personality characteristics, abilities, and interests of the propagandee; and it would consider the manner and medium of the delivery of the propaganda as related to personality characteristics of the propagandee; that is, *it would relate independent and psychological intermediary variables.*

4. Finally, and most crucially, it would endeavor to provide a means for studying how such factors are empirically structured together to produce

propaganda effects; that is, *it would have to contain propositions showing clearly the relationships among variables of the three classes mentioned.*

With such a framework set up, and adequate instruments for the measurement of appropriate variables, it would become possible to conduct a long series of experiments to explore systematically a large number of conditions of suspected importance in the understanding of propaganda. Quite obviously such a program would require theoretical orientation in terms of broader social and psychological theory, and a long series of pilot experiments aimed at developing measuring devices, before serious systematic exploration could begin.[6]

Experimental Investigations of Propaganda-Persuasion Processes

The purpose of this section is to illustrate techniques of research in propaganda and to point out difficulties in this field of research; the section does not present an exhaustive survey of the literature. The section contains three parts: a review of studies of attitude change, a review of studies of action, and a brief summary of problems and shortcomings encountered in research to date.

Studies of Attitude Change

The experimental modification of attitudes typically involves a control group and one or more experimental groups, all equated at the beginning of the experiment with respect to some variable—most frequently the dependent variable (i.e., attitude). Control of differential effects of extraneous variables is most frequently achieved by randomly assigning subjects to groups in the experiment. The experimental groups are then subjected to the predetermined conditions, all groups are compared on the dependent variable some time later, and any differences are attributed then to differences in experimental treatment. Occasionally control groups are not used if the dependent variable is judged to be normally quite stable (**38**). Independent variables applied as experimental conditions have included emotional vs. rational speeches (**21**), majority vs. expert opinions (**31**), favorable vs. unfavorable reading matter (**5**), speech vs. radio address vs. reading (**44**), and various types of school instruction (**22, 16**).

[6] Although there is nothing in print today which begins to meet the requirements of an integrated structural conceptualization, it is the writer's understanding that the current work of Floyd H. Allport and his students is pointed in this direction. (This work is for the most part unpublished as yet, but see **2** for an early and now somewhat out-of-date statement.) A set of hypotheses and a rationale for a general structural approach to the study of behavior are being prepared by Allport for publication.

Reviewing the available studies of relationships between propaganda and attitude changes, one cannot help being struck by the surperficial nature of most of the variables examined. This may be due in part to the apparent (though misleading) ease with which experiments of this kind may be set up and conducted, but probably more to a lack of a systematic theory of attitude. The available studies are almost entirely concerned with what we have previously described as a linear approach, with only occasional effort being made to relate experimentally produced changes to anything other than the manipulations of the propagandist. No report seen by this writer deals with a propaganda experiment using attitudes as dependent variables in which the attributes of the psychological, situational, or structural approaches as described earlier were given serious consideration. The fact of "unreality" of the experiments which Murphy, Murphy, and Newcomb (**35**, chap. XIII) lament is not nearly so important as this lack of study of inter- and intra-individual factors that might be related more importantly to attitude change than most variables made up of symbolic content or other non-coercive social experiences to which the subject in the experiment might be exposed.

If one were to push further Murphy, Murphy, and Newcomb's reservation about unreality of attitude-change experiments, he would have to raise the question of the relationship between attitudes and action. While these experiments are relevant to propaganda definitions stressing attitude and opinion, they offer nothing conclusive about overt action as a result of propaganda. McNemar (**32**) raises the question of validity of attitude measurement, suggesting action as a criterion of validity (p. 296). The typical attitude-change experiment does not raise the question of the attitude-action relationship but tacitly assumes one to exist, attitude being the presumed determining factor. Until more conclusive evidence than we now have is at hand concerning a *causal* relationship between attitude and action we must remain very skeptical of the significance of even the most sophisticated experiments in which attitude measurement comprises the dependent variable.

A study by F. H. Allport and Simpson (**2**) of Axis broadcasts to the United States during the war makes use not of attitude but of degree of belief of assertions in the propaganda, such as (a) that the U.S. was on the verge of defeat and (b) that our allies were aiming to exploit and betray us. The use of a measure of belief as a dependent variable is an interesting departure from previous procedure, but it too needs "validation" against action measures.

An as yet unpublished experiment falling on the borderline between

those using measures of attitude or belief on the one hand and those using measures of action on the other is that of Franklin (17). In this experiment, conducted during the recent war, three groups of college student subjects were given three sessions comprising about ninety minutes of recorded propaganda. One group received United Nations appeals, chosen from those used in the Deren experiment reported on pages 51–53 below; a second group received Axis appeals, selected and edited from German and Italian broadcasts in March and April of 1942; and a third group served as a control group, receiving no appeals. Prior to the propaganda each subject executed a Civilian Participation Inventory comprised of seventy-one items on which he could indicate his participation (or non-participation) in civilian war activities. The subject could indicate his participation (as of the past one-month period), his *intention* to participate in the future, or both, on any item of the Inventory. Between the first execution of the Inventory and the propaganda, the subject copied in longhand (as a part of a concurrent experiment not reported in detail by Franklin) letters to be distributed to other students urging them to discuss and work in the war effort and to urge still others to do so. Following the three propaganda sessions was another letter-writing session after which the Civilian Participation Inventory was executed for the second time.

We need not detail here the kinds of indices obtained from the scoring of this inventory. The results are of interest, however, and are congruent with those reported by Deren (13, see pp. 51–53 below). All three groups increased their *participation* scores on the Civilian Participation Inventory, with differences significant at least at the 5 per cent level of confidence. The gain for the group receiving the United Nations appeals was significant at the 1 per cent level. There were *no significant differences*, however, between the *gains* of the groups. Therefore, the group gains could not have been due to any differences in propaganda content, especially since the group receiving no appeals increased in mean participation score as much as the others. There thus seemed to be other influential factors at work.

When *intention* score differences are considered, much the same general trend is apparent, though the gains were not so high as on the participation measures. The United Nations appeals appeared to be slightly, though not with full statistical significance, more effective than the pro-Axis appeals. The latter were a little more effective than no appeals at all.

Franklin interprets these results as providing "no evidence for the usual naïve belief in the absolute energizing effects of propaganda" (p.

175). She interprets them, rather convincingly, as being due to the effects of what is called "structure." It is noted that *all three* groups (a) executed the Inventory, (b) copied letters aimed at arousing others to war activities, and (c) were members of a nation at war, *before* the propaganda was received. These three things are seen as clarifying the fact that the subject was in a situation structured toward certain outcomes ("victory"). The appeals themselves were insufficient to increase (by comparison with *no* appeals) the amount of one's participation, or intention to participate, in civilian war activities. Nor did arguments directed toward participation in the war effort produce significantly more effect, or any different *direction* of effect, than the arguments designed by the enemy to lessen the subject's participation in the war effort.

Franklin then hypothesizes that propaganda "functions only as a servant of 'structure' [some form of societal intermediary variable], is dependent upon it, and is meaningful only in terms of it. It is at the mercy of 'structure' and thus can be completely neutralized by it" (p. 179). The last sentence in the quotation evidently refers to the fact that pro-Axis appeals also were followed by rises in participation scores.

The participation and intention measures as used by Franklin are interesting departures from previous usages in a realistic direction, namely, toward bringing the measure of the dependent variable closer to the reality demanded by successful propaganda: action.

Studies of Action

Turning now to experimental studies of propaganda in which some form of action is taken as the dependent variable, we find only four: those of Hartmann (**20**), Deren (**13**), Lazarsfeld, Berelson, and Gaudet (**25**) and Merton (**34**). The small number of such studies testifies to the practical and methodological difficulties inherent in them. The amount of time, effort, and money required to discover and measure the significant variables, not to mention the cost of the propaganda itself, is prohibitive. And the proper control of variables once they are discovered is indeed a tricky methodological problem.

Hartmann's (**20**) experiment has been reviewed extensively in other places (cf. **7**, **34**) and so will be treated but briefly here. In the 1935 Pennsylvania elections Hartmann himself was a candidate for office in Allentown, running on the Socialist ticket. There was agreement within the party that the Socialists were quite unlikely to win any offices, making possible the further agreement that nothing would be lost by using Socialist election propaganda in an experimental test of "emotional" vs. "rational" appeals. The experimental groups were the popu-

lation of Wards 2, 9, and 10, which received "emotional" appeals, and of Wards 3, 4, and 7, which received the "rational" appeals. The people in the remaining 12 of the 19 wards served as controls and received no Socialist propaganda whatever. The groups of wards were matched as well as possible with respect to population density, socio-economic status, previous voting habits, size, and assessed real estate values. The appropriate leaflets were handed to the "subjects" by party workers on calls at residences (or left under doors of those not at home). The criterion of effectiveness was, of course, voting behavior. Results are presented in terms of relative gains in Socialist vote in the ward groups over the Socialist vote in the election of the preceding year: the over-all vote increased by 16.69 per cent, with the Republicans increasing 19.79 per cent, the Democrats 13.10 per cent, and the total Socialist vote 30.86 per cent. Experimental group differences are seen in the fact that the Socialist increase was made up of a 50 per cent increase in the emotional wards, 35.42 per cent in the rational wards, and 24.05 per cent in the control wards. Interview follow-ups after the election tended, in terms of recall of the leaflet content, to confirm the superiority of the emotional appeal.

Hartmann himself calls attention to some of the weaknesses of this study, principally the failure to match the "emotional" and "rational" wards with respect to per capita real estate valuation (the evaluation being twice as high in the rational wards), which resulted from matching them on population density. This may or may not be a serious defect in the study, and Hartmann discounts its importance. He points out that Socialist support does not come primarily from the lowest economic groups and that the socio-economic status of the "rationals" was not as high as suggested by the per capita real estate evaluation, since they were families living in upstairs apartments in the downtown business area. Nonetheless, this difference constitutes an unknown determiner of vote differential. Other difficulties which Hartmann does not mention lie in the tacit assumptions of (a) equal readership of appeals, (b) equal occupational representation, and (c) equal convenience to polls, although the treatment of the data in terms of percentage of increase over the preceding year's vote minimizes any importance of the last two factors named. Another shortcoming of this study, one that is conceptual rather than methodological, was the failure to obtain data of psychological and sociological nature about subsamples of his subjects. Such variables may perhaps be more basic to causing an individual to vote the Socialist ticket than types of propaganda appeal.

The Deren (**13**) experiment, as yet unpublished, sought to determine the effects of two classes of variables on output in a typewriting situation in which output was expressed in terms of the number of correct strokes per unit time in copying certain materials. The first class of variables may be called "degree of structurization for attaining impor-

tant ends"; the second had to do with different emotional appeals. The experiment took the following form:

1. A population of 226 students in typing classes of two business schools in Syracuse, New York, first copied a real-estate handbill in the regular class period on days 1 and 2. On the following two days, 3 and 4, the students typed a strongly worded war morale handbill which was to be distributed in various public places in Syracuse.

2. The second phase of the experiment consisted in the delivery, by carefully trained orators, of speeches designed to enhance national morale. Following the numerical scheme used above, days 5 and 6 were occupied with speeches, day 7 was devoted to silent reading of similar material, and days 8 and 9 featured speeches again. Days 10 and 11 were given to further copying of the morale handbill.

The appeal materials were of three basic types and a mixture of two of these types. A survey of propaganda in the press around the time of the experiment showed that most of it could be classified under one or another of the following headings: (a) *Avertive*, calling up emotions of fear, hatred, or rage and making the recipient want to keep the results of losing the war as far from him as possible; (b) *Devotive*, arousing emotions of sympathy, loyalty, or affection toward American or Allied soldiers and making the recipient want to support and share their efforts; (c) *Meliorative*, arousing feelings of altruism toward all mankind and making the recipient want to end the war so that all might enjoy a better world. The first two categories often appeared together in public print.

Appeals of equal length in all three basic categories were specially prepared for the experiment and were judged for strength, or moving-quality, by a panel of judges. They were then revised, further edited, and rejudged. A combination *Avertive-Devotive* appeal was made from the originals of those classes and also judged for strength. In the final forms there were no reliable differences in judged strength among the four sets of material.

The subject population was divided into five groups, one to receive each of the four sets of appeals, and a control.

The comparison of performances on days 1 and 2 vs. days 3 and 4 was to reveal the effects of being structured into a situation (or "national morale system," to use the language of the report) wherein the products of one's work could have important and clearly understandable bearing on maintaining the functioning of social structures important to the worker. The results of this phase of the experiment showed no differences that could be reliably attributed to the change in the structuring of the situation despite the fact that on later questionnaires most subjects reported feeling like working harder when typing the morale handbill than when typing the real-estate handbill. The absence of effect may have been due to the typically high motivation in the typing of

classroom exercises (i.e., effects of being in the classroom structure) regardless of the content of what was typed.

The comparisons of scores on the morale handbill typing on days 10 and 11 with days 3 and 4 again showed essentially negative results: no increase in correct typing strokes could be reliably attributed to any of the kinds of appeals or to the appeals taken altogether. Deren concludes with the suggestion that propaganda in the form of emotional appeals has little or no effect in altering behavior output of people in structured stiuations. Inasmuch as this experiment was exceptionally well controlled, this suggestion needs to be taken seriously and investigated with care.

The search for more basic variables received considerable impetus through the study of Lazarsfeld, Berelson, and Gaudet (**25**) of "how the voter makes up his mind in a presidential campaign." In this study, almost sure to become a classic and a point of departure for improved ones yet to come, the stratification techniques of the public opinion pollers were used to sample the population, and some interesting indices were developed in an effort to find important conditions of the population that would be related to voting intention and to voting behavior. Erie County, Ohio, was chosen as the locale for this research because of its convenient size, its freedom from sectional peculiarities and from domination by a large urban center, the possibility of comparing rural with small urban political trends, and because of the fact that its distribution of votes among the various parties had for forty years closely approximated that of the entire country.

Their method of obtaining data was as follows: An initial poll of 3000 eligible voters was made in May of 1940, in which basic sociological data as to occupation, religion, socio-economic status, etc. were obtained. From this original number four groups of 600 each, equated with respect to the mentioned variables, were formed. One was arbitrarily designated a panel, and re-calls were made to its members each month thereafter, the last interviews being taken shortly after the election. The other three groups were controls and were interviewed again only once: one group in July, one in August, and one in October. The panel method thus gave continuous information on changes in voting intention and also provided an opportunity to secure a great amount of additional information about panel members. The control groups served as checks on the effect (which did not materialize significantly) of repeated interviewing on voting intention and behavior.

Important in the present context is one of the several indices and other ways of describing the population that these researchers developed. First, ratings of socio-economic status (SES) were made, using five categories with proportions of the population as indicated: A, 3 per cent; B, 14 per cent; C+, 33 per cent; C−, 30 per cent; D, 20 per cent (A being the highest and D the lowest). Party membership was found to vary

with SES, religious affiliation (Protestant and Catholic only), rural vs. urban residence, and, to only a slight extent, age. From these findings an Index of Political Predisposition (IPP) was developed. This index ranged over six steps from strongly Republican (1) to strongly Democratic (6), and was derived from the values in Table I. To illustrate: a

TABLE I

VALUES INCLUDED IN LAZARSFELD, BERELSON, AND GAUDET'S INDEX OF POLITICAL PREDISPOSITION (25)

	Protestant		*Catholic*	
SES	Rural	Urban	Rural	Urban
A, B	1	2	3	4
C+	2	3	4	5
C−	3	4	5	6
D	4	5	6	7*

* Sevens were lumped into the sixth IPP step.

person classified C− in socio-economic status, who is Protestant and lives in the country, would have an IPP of 3, as would a city-dwelling Protestant classified C+ in socio-economic status.

Turning to the effects of propaganda, we note that a political campaign may have different kinds of effects. When we speak of "the effect of propaganda" we usually have in mind a change from one position to another on some scale; and whether we get this type of effect depends in part on the scale we use. If our scale is dichotomous, as voting Republican vs. voting Democratic, the only change-in-scale effect possible is outright *conversion*. But another possible effect is what these writers call the *activation effect*, in which the campaign activates the political predisposition of "crystallizers" (those people undecided in their vote intention early in the campaign). Such people were found usually to act in accordance with their IPP's. Still a third effect is called the *reinforcement effect*, meaning the strengthening of original vote decisions, keeping partisans in line, and reducing defections from the ranks. The data on this third type of effect are principally qualitative and were secured in just-before-election interviews. The "pure conversion" effect resulting entirely from Republican propaganda on some campaign issue (for example the third-term issue) must meet these criteria: (1) Voted Democratic in 1936, (2) did not intend to vote Republican before the Democratic Convention, and (3) believed Roosevelt had been a good president. The report states that less than 2 per cent of the total Republican vote in Erie County was attributable to conversions by propaganda on this issue. A few additional people ($N=14$) are reported as

being converted by the argument that "Willkie is the champion of the poor," and a very small number—not even reported—of conversions were of voters who conformed to the stereotype of the rational citizen withholding judgment until all arguments were weighed. These people, incidentally, proved to have "weak" IPP's (while they met the three criteria mentioned above, their IPP's were neither Democratic—5 and 6—nor strongly Republican—1 and 2).

This excellent study, making use as it did of the best techniques available at the time for analyzing sociological variables, stands as the prototype of the situational approach to propaganda. Within the limitations of their methods and facilities these researchers even attempted to obtain relevant information on the personalities of the voters, though without much success. This fact suggests their awareness of what we have called the structural approach. One can easily imagine the practical obstacles to an intensive study of personality within a population of any size; the methodological problems which such an undertaking would add to a study like Lazarsfeld's would indeed be formidable. But with sufficient time, a staff of a hundred or more psychologists and sociologists, and a budget increase of several hundred thousand dollars it might be done. These estimates are made merely to suggest the magnitude and difficulties of other than exploratory work of this type.

Having raised the question of the effect of personality characteristics on response to propaganda in an election, we turn to another study of "mass persuasion" which goes further into a consideration of personal motives and their relation to propaganda effects. We are referring to Merton's (**34**) report of Kate Smith's efforts in her third all-day radio marathon to induce people to buy war bonds. Although this study did not give as much attention to sociological variables and was not as well controlled as the Lazarsfeld study, it does show many instances of war bond purchases due to, or at least precipitated by, the urgings of this radio star. It is probably an exaggeration to say that she "succeeded in selling some $39,000,000 of war bonds in the course of one day's broadcasts." It is an exaggeration on conceptual grounds in that there were many factors other than Miss Smith's appeals contributing to the sales, as the report clearly shows, and as Boden (**9**) has also pointed out. It is an exaggeration on methodological grounds because adequate pre-broadcast control data on bond buying tendencies were not secured and because the total sales figure attributed to Miss Smith was an extrapolation from data obtained in New York City where the broadcast was originating. Nonetheless, some very interesting and probably important insights into the workings of a propaganda campaign were uncovered. Let us see how they were obtained.

At eight in the morning on September 21, 1943, Kate Smith began a series of appeals on behalf of the Third War Loan, a series made up of sixty-five separate appearances and extending to 2 A.M. the following day. The pro-

cedures for studying Miss Smith's effectiveness contained three major features. First, methods of content analysis (cf. 26) were applied to the scripts used by her in an effort to discover, via the second feature, the focused interviews, the most effective appeals. The focused interviews were conducted by trained personnel in the homes of 100 respondents, lasted three to four hours, and were of the "non-directive type," the respondents being encouraged to report their own foci of attention. The respondents were 75 people drawn from the lists of those who telephoned bond pledges into radio station WABC during the day of the broadcasts plus 25 additional people who had heard the Kate Smith broadcasts but did not respond with a bond purchase. These focused interviews produced data suggesting hypotheses regarding popular images of Miss Smith and other possible motivational factors. These hypotheses could not be tested for validity in the public at large, since data from only 100 intensive interviews were used. Therefore, polling interviews of 978 people in a cross-section of the Greater New York population were conducted to secure additional data. Merton is not clear as to just when the interviews took place. The focused interviews began the day following the broadcast. Half of them were completed within a week and "the bulk of" the others during the week following. No information is given as to the time of the polling interviews. The delay of many of the focused interviews constitutes an additional uncontrolled variable in the observations, for it is well known that original impressions can change greatly with time and depend in unknown ways on personal characteristics (4).

Merton's analysis of interviews and content brings out several characteristics closely relevant to our outline of a structural conceptualization (see pp. 46–47 above). First, there is the organization and content of the persuasive arguments—different appeals evoke different responses and lead to different decisions. Second, the relationship between the propagandist and the propagandee is emphasized, in the form of evidence of the importance of attitudes toward and images of Kate Smith in effecting the decision. Third, the predispositions of the listeners, comprised of a "general orientation" to buy war bonds and a "specific attitude" toward buying more bonds in the Third War Loan drive, are recognized as an important determinant of the effectiveness of the propaganda.

One additional factor which Merton does not recognize as independent of the persuasion situation per se, but which on more careful analysis may be so recognized, is the availability of means for action according to the urgings of the propaganda. The Kate Smith drive allowed the listeners to telephone pledges to their radio station with the understanding that they would later be seen for the actual purchasing transaction. We may be fairly sure that the drive would not have been nearly so successful without this convenient arrangement through which the desired action could be effected (e.g., if people had been asked to run down to

the nearest drug store to sign a pledge). We conceive such an arrangement as this not as a part of propaganda itself (i.e., of the independent variable), but as an adjunct to propaganda, a societal intermediary variable, necessary for attaining the objective of the propaganda.

Summary of methodological problems

Now let us point out some of the problems in propaganda research that are illustrated in studies reviewed or referred to above.

1. *Sampling.* Twenty-eight of the thirty-four attitude change experiments reviewed by Murphy, Murphy, and Newcomb (36, chap. XIII) used college or other students as subjects, a limitation on the generality of findings, particularly serious even in exploratory studies when difficulty No. 2 is not met.

2. *Choice of significant independent and intermediary variables.* Practically all studies of attitude change, and Hartmann's action study (20), give most attention to content or its mode of delivery and very little to concurrent conditions within and surrounding the subjects. More fruitful hypotheses for future study would probably have resulted from these experiments, even with the limited population used, had theory and method been more broadly conceived.

3. *Choice of significant dependent variables.* Attitude changes in themselves are of unknown significance with respect to later action. While the difficulties of obtaining action criteria of propaganda effect are appreciated, it seems that in many experiments the question has not even been raised. Flowerman's (18) article suggests a number of measures in addition to attitude that might serve as useful dependent variables.

4. *Adequate measurement and control of independent and intermediary variables.* Most attitude change experiments compare different features of propaganda without effort to control other possibly important aspects of the material. Sims (39), for example, reports different effects on attitudes toward the TVA of pro-utility and pro-TVA propaganda, but his experiment does not control the length, number of themes, quality, or other attributes of the materials, simply selecting some already prepared material at hand. This and other shortcomings also limit Menefee's (33) study of propaganda in the Washington lumber strike of 1935. Such variables making up the main conditions of a propaganda experiment are indeed often hard to measure or control. But their measurement and control are absolutely crucial in answering a question via experimentation—they even determine what question *actually is* being answered by the experiment. Thus, in a research area such as propaganda, where there are many practical difficulties in the way of experimentation and where there is little clear-cut basic knowledge to begin with, the experimenter must be all the more careful to have adequate knowledge of and control over the conditions of his research.

5. *Adequate measurement and control of dependent variables.* The nuances in this problem need fuller appreciation. Attention has already been called to the fact that Merton's (34) interview data were gathered variously over a greater than two-week period following the propaganda, an understandable but regrettable fact. Another noteworthy condition affecting the dependent

variable in some experiments is the attitude of conformity (cf. 1, pp. 298–300, and 43) which operates in group situations to restrict the range of measurements; this condition is likely to vary with different classroom situations (if, indeed, measures *must* be obtained in classrooms). When interviews are the sources of data, special care must be taken to ensure that interviewers are highly and *equally* trained (this applies, of course, to the obtaining of data on intermediary variables as well). In the absence of conclusive evidence of validity in measures used as dependent variables, the conclusions from propaganda experiments should be very carefully and thoroughly qualified, lest misunderstanding and further confusion in propaganda theory and methodology arise. Indeed, a very worthwhile series of experiments could deal directly with the development, refinement, and thorough understanding of dependent variables for propaganda research.

6. *Budget.* One occasionally finds reference in propaganda experiments to the fact that the cost of certain procedures would be prohibitive. No experimenter can be blamed for the high costs of his operations. Caution can, however, be exercised in some cases to keep from spreading funds so thinly that the experimenter loses control over his conditions or is unable to tap variables of possible significance. Consider, for example, the possible differences in results Merton (34) might have obtained had he hired more interviewers to gather data more quickly after the propaganda instead of using fewer interviewers over a longer time, or if Hartmann (20) had spent some of his funds getting better control over sociological variables and less on the leaflets, or if Lazarsfeld (25) had obtained more information on personality variables while spending less money on control-group polling. The upshot of all this is that research in this branch of social psychology is extremely expensive if it is to be done well, and in the absence of truly adequate funds experimenters have some obligation to allocate what they do have so as to yield the most meaningful and valid data.

Summary

We have pointed out that there is as yet no well established body of systematic fact and theory under which the phenomena generally termed propaganda can be subsumed. In an effort to establish this point we have reviewed eighteen discussions of propaganda phenomena. It seemed possible to classify them in the following broad conceptual groups:

1. *Linear conceptualizations*, which seem to view propaganda as a set of forces (or independent variables) inherent in the propaganda materials themselves, which intermingle with other social forces in a sort of ideological tug-of-war causing people to do now one thing and now another (dependent variables). Intermediary or explanatory variables tend to be overlooked.

2. *Psychological conceptualizations*, which emphasize as intermediary variables and as dependent variables the importance of attitudes, values, personality characteristics, or the psychological field in determining the effectiveness of propaganda.

3. *Situational conceptualizations*, which give most attention to the objective facts of social organization as intermediary variables, rather than to values or

attitudes arising from them. Some such treatments were shown to have strong "psychological" admixtures, but the writer's judgment of the relative emphases determined to which category the treatment primarily belonged.

4. *A structural conceptualization* was suggested in which the interdependent functioning of objective social structures and personal characteristics of the propagandees is seen as a complex of intermediary variables determining, and at the same time mediating, the effects of propaganda.

The second major section of the paper reviewed efforts to study propaganda experimentally and pointed out problems in this field of research. Experiments aimed at changing attitudes were treated first and it was concluded (a) that such experiments have on the whole studied the effects of rather superficial independent variables and (b) that change in attitude scale position is of questionable significance as a dependent variable. Four efforts to study the influence of propaganda upon action rather than attitude as the dependent variable were reviewed. These studies highlighted techniques and problems typical of efforts to study propaganda experimentally in "real life" situations. This section closed with a discussion of six important types of problems which must be considered in any kind of propaganda experiment: sampling, choice of significant independent and intermediary variables, choice of significant dependent variables, adequate measurement and control of independent and intermediary variables, adequate measurement and control of dependent variables, and budgeting.

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A CRITICAL REVIEW OF THE VALIDITY AND RATIONALE OF THE FORCED-CHOICE TECHNIQUE

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According to a paper by Staff, Personnel Research and Procedures Branch, the Adjutant General's Office (**5**), the basic idea for the forced-choice technique was developed by Paul Horst with reference specifically to personality scales. Robert Wherry developed a similar idea while working on personality measurement for the Civil Aeronautics Authority, and later the Staff of the Personnel Research Section of the Adjutant General's Office attempted to apply the concept first to the design of personality inventories and later to the problem of rating officers. The latter application resulted in the production of the Army Efficiency Report, which was used for some years for the official rating of officers. More recently the technique has been used by various professional persons and for a number of different purposes. The method of making a forced-choice scale as used by the Personnel Research Section, has been well described by Sisson (**3**).

The general nature of a forced-choice rating scale is best understood in terms of the procedure which is followed in building it. The following are the essential steps involved:

1. Descriptions are collected of individuals who are at each extreme of the scale to be measured. If the scale is that of effectiveness as a supervisor, descriptions are obtained of the most effective and the least effective supervisors. This procedure partly defines the scale that is to be measured by the final instrument.

2. The descriptions collected are then dissected into a list of small elements. Each one of these elements describes, in essence, a rather specific item of behavior. The complete list should cover all the important aspects of the job, and the number of items covering each aspect should be related in some rational way to the importance of that aspect.

3. Two values are determined for each item listed. One value, the discrimination value, indicates the degree to which the item measures the particular characteristic that is being measured. The other value indicates the extent to which individuals tend to rate others too high or too low on the particular characteristic. This latter is sometimes referred to as the preference index or preference value of the item. Both values are determined experimentally.

4. The characteristics are then arranged in pairs such that the two members of each pair differ in the extent to which they discriminate. Ideally, one of the characteristics in each pair should have a discrimination value of zero and one

should have a high discrimination value. Also, it should be impossible for those who are to use the scale to determine by inspection which one of the two characteristics is the discriminating one.

5. The pairs of characteristics may then be grouped in fours, with each group of four including two desirable and two undesirable characteristics. The main purpose of grouping the characteristics in groups of four seems to be that persons filling out the scale may have a better attitude towards it if the grouping in tetrads is adopted. This seems to be the only advantage of the tetrad over the duad form. A pentad form has also been suggested.

6. Directions are prepared in which the individual is instructed that he is to examine each group of four characteristics and to select the one that is most characteristic and the one that is least characteristic of the person who is being rated. The person filling in the form is required to make these choices.

7. The selection of the items is then validated against an external criterion on a sample that was not used in the original procedure for selecting and pairing the items.

The scoring system is such that the person who is being rated receives a positive score if the item which is most descriptive of him is a discriminating desirable characteristic or if the item least descriptive of him is the undesirable discriminating item. The scores on items may be weighted in some complex manner or the weights may be restricted to the values of zero and unity.

What Does the Rater Do in Completing a Forced-Choice Rating?

The task performed in rating another person on a forced-choice scale is somewhat different from that performed in any other phase of life. Consider, for example, the simplest situation in which two items are presented and the rater is asked to decide which one is most characteristic of the person rated; and suppose that the scale was to be used for evaluating some phase of administrative ability and that the following items constituted the choice offered:

Enjoys making speeches at company dinners
Contributes to company magazine

Suppose that the rater has good evidence to show that the person being rated has a matter-of-fact attitude toward the task of making speeches at company dinners. He does not get any particular enjoyment out of speech-making, but neither does he find the job distasteful. As far as contributing to the company magazine is concerned, he dutifully writes an article every year, as do most executives of the particular company. The situation is confusing for the rater, to say the least, since he is required to compare an item of behavior which varies in degree with one which varies in frequency. Yet the confusion in this instance is much less than that produced by some of the items in the

Army Officer Efficiency Report. In the latter connection, consider the following tetrad of items which appears in the Army scale (WD AGO Form 67-1 Part 2)

A go-getter who *always* does a good job
Cool under *all* circumstances
Doesn't listen to suggestions
Drives instead of leads

In the example, the two words italicized were not in italics in the original version. If these two words are carefully examined in content it will be found that their inclusion makes it impossible to compare these items with the others. Quite obviously a man either is or is not cool under *all* circumstances. It is not an item of behavior which varies in degree. Under such circumstances the rater is asked to perform an impossible task. He cannot say that one of a set of items is more characteristic of the person being rated than any of the other items listed, because some of those listed either are or are not characteristic of the person being rated regardless of the other items in the tetrad.

This confusing situation is mainly a result of the way in which forced-choice scales have been constructed and is not a necessary consequence of the forced-choice technique.

How Can the Choice Be Made Rational?

It is anticipated that one of the necessary changes in the method in order to develop its usefulness as a measuring device will be to modify it in some way so that the rater makes choices only in situations in which choice is rational. One step in this direction would be to include in each tetrad only items of behavior which vary in frequency of occurrence within a given individual or only items which vary in degree. It hardly seems possible to compare behaviors which vary in frequency with behaviors which vary in degree.

The second necessary step in clarifying the method of measurement is in defining what is meant by the "most characteristic" or "least characteristic." Like many terms, these seem clear until they are carefully examined for meaning. For example, when a rater is asked to select from two items of behavior the one that is most characteristic (or most descriptive, or whatever else), he is really being asked to choose the one on which the individual deviates most from the average or from some other standard. If two items of behavior which vary in frequency are under consideration, a comparison of the frequency of one item of behavior must be made with the frequency of occurrence of the other item of behavior in the person rated. This comparison should presumably be made not in terms of absolute numerical frequencies but in

terms of some converted score which takes into account the relative scatter of scores of the two variables. The rater might be asked to choose the characteristic on which the individual would have the highest relative standing in his group (which would have to be defined) if all individuals were ranked in order. If the directions could convey this kind of concept it might help to bring meaning into a confused situation.

However, having developed and clarified the psychological concept to this point, the reader will probably have already jumped ahead to ask the question "Why is it necessary to force the rater to choose between the characteristics? Would it not be simpler to ask him to rate the individual on each one of the two characteristics and then examine his ratings to determine which one of the two characteristics was rated higher?" The answers to these questions are in the affirmative, since, if the directions are clear, it is necessary for the rater to assess the individual rated on each one of the items of behavior listed before he can rationally determine "which one is most characteristic" or "which one is most descriptive." The only additional restriction placed by the forced-choice method is that the rating on all the characteristics in a given group must be different, otherwise the forced-choice scoring system cannot be used in its present form.

If the rater is asked to make an assessment of the individual to be rated on each of four characteristics, but with the added restriction that no two ratings within any group of four may be placed at the same point on the scale, the resulting record may be scored in exactly the same way as any of the forced-choice scales at present in use. In other words, the process of forcing a choice is an unnecessary and irrelevant part of the procedure. However, what is left when the procedure is rationalized in the way shown is a scale in which tendencies to overrate or underrate on relevant qualities can be corrected by overrating or underrating on irrelevant qualities. It is assumed in this procedure that, for any given individual, the true rating on the irrelevant items is average and that any deviation in the average ratings on these irrelevant characteristics represents a tendency to overrate or underrate the particular individual who is being rated. The relevant items can be selected empirically, in the first place, to make this assumption acceptable. The procedure is simply that of using ratings on certain characteristics as a suppressor variable to correct for errors in the rating on certain other characteristics. In this form the basic idea of a forced-choice scale seems to have considerable merit and to be one worth developing, though possibly in forms other than the stock-in-trade forced-choice instrument at present in use.

The Validity of the Forced-Choice Rating Scale

The claims for the validity of the technique seem to bear little relationship to the actual evidence.

A primary claim is that "it produces a better distribution of ratings relatively free from the usual pile-up at the top of the scale" (3). The data in Figure 1 were presented to substantiate this claim:

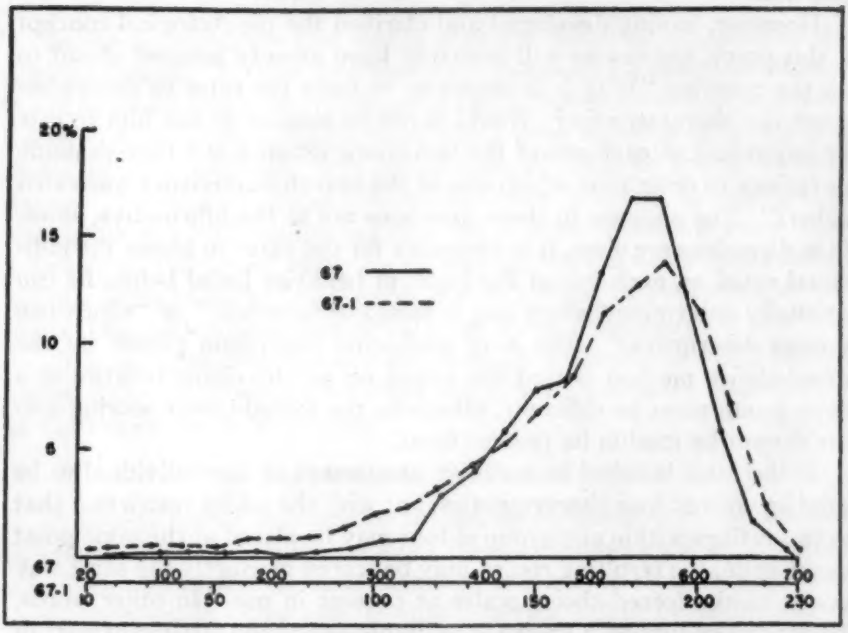

FIG. 1. DATA PRESENTED BY SISSON SHOWING DISTRIBUTION OF RATINGS ON CONVENTIONAL RATING SCALE FORM 67 AND DISTRIBUTION OF RATINGS ON THE FORCED-CHOICE RATING SCALE FORM 67-1 (3, p. 375).

Figure 1 is supposed to compare the distribution of ratings of officers when the old conventional rating scale (Form 67) was used and when the new forced-choice rating scale (Form 67.1) was substituted. It is quite obvious to anyone who examines the above graph that differences in the two distributions do not at all substantiate the claim that the forced-choice rating technique is "relatively free from the usual pile up at the top of the scale." This claim is even more ridiculous when it is substantiated by Figure 2, which presents the contradiction of stating that it is based on data in which range is equated; inspection of the data seems to indicate that the ranges of the two sets of ratings were not equated (data from J. C. Fry).

A second claim is that the forced-choice rating scale as used by the

Army "produces ratings which are more valid indices of real worth" (3). Similar unsubstantiated claims come from Richardson (2), but the apparently spurious nature of the validities presented by him and his group will be discussed later. Claims contradictory to the above are also made by professional personnel who worked on developing the Army Report Form, as is exemplified by the following statement by

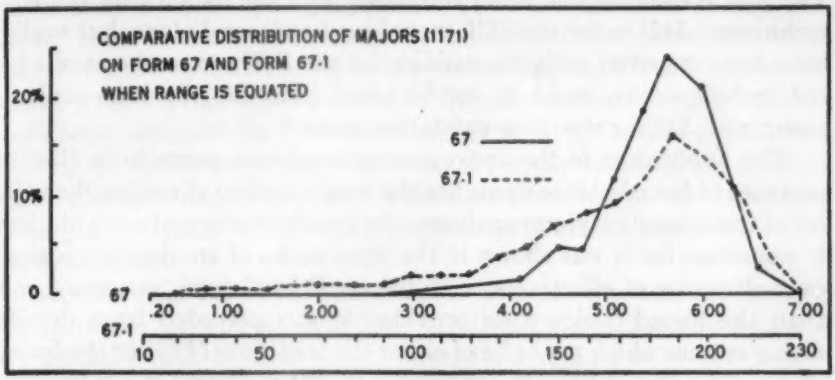

FIG. 2. DATA PRESENTED BY FRY SHOWING DISTRIBUTION OF RATINGS ON CONVENTIONAL RATING SCALE FORM 67 AND DISTRIBUTION OF RATINGS ON THE FORCED-CHOICE RATING SCALE FORM 67-1 (1, p. 22).

Staff, Personnel Research and Procedures Branch, the Adjutant General's Office (4):

> A single over-all rating on a 20 point scale, provided it is preceded by a series of specific ratings—using either the paired or unpaired items that have been described—is more valid than either type of specific item alone.

Claims for superiority of the forced-choice rating scale and claims for the superiority of traditional methods coming from the same research office lead one inevitably to conclude that available data cannot give strong support to the superiority of the one method over the other in so far as Army studies are involved. The following quotation summarizes all the statistical evidence that the present writer has been able to obtain concerning the relative validity of the forced-choice (FCL) and the more traditional type of rating device (RCL) as it is used in the Army Officer Efficiency Report (4):

> The FCL key contains 48 items; the RCL key, 93 items. On the basis of length of rating scale the RCL key would be expected to yield the highest criterion correlations. This is the case for the items scored within the OER-B form, although the difference for the larger group is slight (r's equal .502 for FCL score and .513 for RCL score). When the FCL form and the RCL form are given separately, however, its difference favors the FCL form (r's equal .466 for FCL and .345 for RCL). These findings suggest that when used alone

the FCL technique is superior to the RCL technique. That the two techniques yield similar results when used in combination may well be owing to the forcing of more serious consideration of the individual items when it is known that a choice must be made after rating on each single trait.

Combining the two techniques yields the highest validity for both the item-analyzed group (.540) and the cross-validation group (.562). The gain is considerable when compared with the validity for the RCL technique (.345) or for the FCL technique (.466) used alone, but negligible when compared with the validity for the RCL score obtained when the techniques are used in combination (.513 for the item-analyzed group and .535 for the cross-validation group).

The implication in the above quotation almost seems to be that the presence of forced-choice items has the magical effect of raising the validity of traditional rating procedures. Any such interpretation is obviously nonsense, for it was shown in the same series of studies that a single over-all rating of effectiveness, of the traditional type, was more valid than the forced-choice scale provided it was preceded by a detailed rating system which might be of either the traditional type or the forced-choice type. What the data do show is that the validity of a rating scale is likely to vary to a marked degree with the orientation given to the rater. The data suggest that experimentation with different types of directions may yield much more important results than experimentation with forced-choice scales.

In whatever way these matters may be interpreted, the fact remains that the traditional type of detailed rating procedure and the forced-choice procedure have approximately the same validity when orientation is given; and an over-all rating scale of officer effectiveness may have higher validity than either of these procedures.

Other Validity Studies

Other material presented as evidence of validity is supplied by Richardson (**2**). The present writer cannot accept the evidence provided by that author and his associates since the procedure involved seems to raise spuriously correlations between assessments of job performance based on a forced-choice scale and an independent criterion of job proficiency. The Richardson technique seems to result in spuriously high validities because of the technique used for refining the criterion. The criterion of supervisory ability is obtained from judgments of those who are in a position to judge the supervisors. Where judges disagree on the rating to be assigned to a supervisor, Richardson discards the case. What is happening, of course, is that those supervisors who are at the extremes of the rating scale tend to be retained while those in the

middle of the distribution will tend to be rejected, since those at the extremes can almost inevitably be more accurately judged than those in the middle range. The effect of Richardson's procedure on the distribution of cases on the criterion measure must be considerable since he reports that 51 per cent of the cases were rejected. It need hardly be pointed out that a correlation between the criterion and the forced-choice scale is almost inevitably raised by increasing the relative proportion of cases at the extreme ends of the distribution, particularly when the reliabilities of the criterion rating varied from only "0.4 to a (chance) one of 1.00." It should be noted in this connection that the validity coefficients supplied by Richardson apply to hypothetical populations of cases on which raters agree and do not apply to the population on which the scale was to be used. Such data provide little evidence concerning the validity of the scale when used for the purpose for which it was designed.

Another point to note in connection with the Richardson study is the use of the concept of reliability. While the study shows that a rater is consistent with himself—both throughout one form of the forced-choice scale and from one form to another—in the only case in which different raters used different forms of the rating sheet the reliability of the resulting rating was only 0.69. Also it is not clear whether this reliability was determined on the attenuated or on the original population before cases were discarded. If this particular reliability coefficient, and the others for that matter, were calculated on the basis of the reduced population (with 51 per cent of all cases discarded), they would probably be numerically larger than if the entire population was used.

Other Criticisms of Validity Studies

There is, however, a basic criticism which must be made of all the validity studies which have been reviewed, namely, that they all involve the unsatisfactory process of predicting ratings with ratings. Studies which involve criteria other than judgments must be made and must form the ultimate basis for evaluating the effectiveness of various assessment techniques.

Another claim made by the protagonists of the forced-choice technique is that it prevents the rater from controlling the value of the assigned rating. It is fairly obvious, however, that this is not so. If a rater decides he wants to give a person a higher rating for job proficiency than is deserved, all he has to do is to think of the person who was generally considered to be most proficient in that job and to fill out the forced-choice form for that person. The technique would almost certainly ensure a high score. In a similar way a low score could be assigned

at the will of the rater. The method which a rater has to use to control the rating is a little more complex with the forced-choice technique than with the conventional rating technique. It is still an open question as to what fraction of raters will see this loophole in the technique, but loopholes of this kind are likely to become common knowledge.

General Summary

A careful examination of the forced-choice technique as it is used in the Officer Efficiency Report reveals serious defects in the rationale which result in the rater having to make judgments which are strictly illogical. These defects in the technique are not, however, basic and can be remedied.

An examination of the validation studies of forced-choice assessment methods reveals that the evidence does not support some of the claims made for the validity of these procedures. The results show that in studies in which little criticism can be made of the procedure, there seems little to choose between forced-choice and traditional rating. The high validity coefficients secured by Richardson and his associates must be considered to be largely spurious until they are demonstrated to be otherwise.

While validity data published at the time of this review must be considered extremely unsatisfactory and, on the basis of this evidence, the technique does not seem particularly promising. Proper studies need to be made to determine the validity of scales in this area constructed on the basis of an adequate rationale.

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"TASTE BLINDNESS" TO PHENYLTHIOCARBAMIDE AND RELATED COMPOUNDS[1]

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A recent paper by Cohen and Ogden (7) raises a number of points which seem to deserve some comment.

1. The authors attribute the discovery that different substances may taste different to different people to Lasselle and Williams (11, 19). It is true that these authors reported that creatine is tasteless to some persons and intensely bitter to others. However, there have been no published observations confirming this. When the note by Williams (19) appeared in *Science*, I tested about thirty persons without finding one who found the substance bitter. More recently I have tested forty-three persons here, partly Americans and partly Egyptians, and found eight who pronounced the taste "slightly bitter." Nobody seemed to perceive the intense bitter taste which phenylthiocarbamide (PTC) has for those who can taste it. Individuals who taste creatine as intensely bitter must be rare, and of course this taste reaction is not controlled by the same gene which controls the ability to taste PTC.

2. In discussing the possible relationship of the saliva to ability to taste PTC, the authors seem to have overlooked the careful study by Hartmann (10), who found that non-tasters could not taste PTC even when it was dissolved in the saliva from strong tasters.

3. Cohen and Ogden imply that sex differences in ability to taste PTC are not significant statistically. In our own work (4, 5) differences which were definitely significant were found in several populations. For example, the differences shown in Table I are all statistically significant and in the case of the third population the chances that the observed differences could be due to errors of random sampling are less than one in a million. This sex difference had already been observed by Fisher in England and is shown in his large series of tests, published by Falconer (8). It is true that in some populations no significant sex difference in tasting is observed, but we must allow for racial variations, which undoubtedly affect the frequency of the gene for tasting PTC, and also the degree to which the expression of the gene is influenced by sex. Ridell and Wybar (18) also refer to the sex difference. Falconer's paper, which also shows the difference, is not mentioned by Cohen and Ogden. In fact, the table presented by Cohen and Ogden and called a "summary of the extensive investigations by interested scientists" does not give the results of Ridell and Wybar, of Fisher, or our own results, and thus

[1] The opinions expressed in this article are those of the author and are not to be construed as reflecting the views of the Navy Department.

omits information about quite a number of populations in various parts of the world.

4. Cohen and Ogden state: "It was hoped that the numbers would form rough racial clusters in the same manner as blood groups. . . . Unfortunately, no ordering seems to result. Indeed the data themselves are internally inconsistent. . . . This indicates that the experimental procedures are weak. . . . Almost all the studies . . . merely describe populations and give results; there is little discussion of experimental technique." This may be true of the results collected by Cohen and Ogden, but these represent less than half of the published studies, and many of them are very early work. In our own papers the technique of

TABLE I

NUMBERS OF MALE AND FEMALE TASTERS IN THREE POPULATIONS

Group		*Tasters*	*Non-Tasters*
Dublin	Males	11	10
	Females	150	49
Zagorsk	Males	114	83
(near Moscow)	Females	194	95
Wales	Males	56	69
(North Towns)	Females	83	29

* Sex differences in tasters are significant in all three groups.

testing is carefully described; it is described by Falconer, and by Matson (12), whose results are also omitted. Obviously tests for something subjective, such as the sensation of taste, should be conducted with care, and many precautions are necessary to insure accuracy.

In our work we have always used "blanks" to test the accuracy of the answers, and have sometimes used paper impregnated with quinine and paper impregnated with saccharin, to get known positive responses. It is also a great advantage if the investigator can converse in the native language of the subject, since work through interpreters is often difficult. In testing the Navaho Indians (6) it proved necessary to learn enough of their language to ask the questions and understand the answers, as no interpreters with a sufficient knowledge of both Navaho and English taste words could be found. In fact, many persons do not correctly understand the meaning of the word "bitter" in their own language, and when a subject reports that PTC tastes bitter to him, it is always necessary to ask "bitter like what?". Unless the substance used for comparison is really bitter, it is necessary to conduct further

tests before classifying the subjects. If adequate precautions are taken, nearly all individuals can be classified as "tasters" or "non-tasters," and reproducible results are obtained when successive samples of the same population are examined. Failure of some of the earlier workers to appreciate this fact probably accounts for most of the "internal inconsistency" in the data.

Even in the incomplete table presented by Cohen and Ogden, definite racial differences in the frequencies of tasters can be found. For instance, the results on the white Americans examined by Snyder (17) (and it will be noted that Snyder's two samples differ very slightly) are so different from those obtained by Rikimaru (14, 15, 16) on the aborigines of Formosa that the value of chi-square calculated from them is of the order of 500, so great that it cannot be found in Fisher's tables (9) of chi-square. Other marked differences appear in the table. Also the studies on the Navaho (6) and the Indians of Montana and Alberta (12) make it clear that well-marked racial differences in ability to taste PTC exist.

5. Finally it may be mentioned that in addition to di-phenylguanidine (18) and thiouracil (13), already known to be tasted as bitter by the same persons who can taste PTC, it has been found in this laboratory that ability to taste various other related compounds is controlled by the same gene (3).[2]

It is also of interest to note that the bitter taste of these various compounds is of different intensity. Among the least bitter are l-5-vinyl-2-mercapto-thiooxazolidone and thiouracil. Phenylthiocarbamide is much more bitter than these, or than thiocarbamide itself, and the imidazole[3] derivatives give the most intense sensation of all, so much so that a few non-tasters for PTC can taste them as faintly bitter, after some delay. Evidently the taste gene, like many others, differs in "expressivity" in various individuals. Such differences have been established (2) even in the case of the blood group genes, which are among the genes having the most uniform degree of expressivity.

Summary

Evidence has been presented that statistically significant differences exist in some populations between the percentages of the two sexes which are able to taste phenylthiocarbamide (PTC) as bitter, and that significant racial differences exist. Some of the problems of testing have been discussed. The fact that certain other compounds somewhat similar in structure to PTC are tasted as bitter by the same persons is discussed, and results with several new substances are mentioned.

[2] The author and D. O. Hoffman have in preparation a report of additional results relative to this problem.

[3] To be reported by Boyd and Hoffman.

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JOHNSON'S REVIEW OF *WORK AND EFFORT*

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To anyone who has studied Ryan's penetrating analysis of the problems of fatigue and efficiency,[1] Johnson's review of *Work and Effort* must come as a distinct surprise.[2] There is little apparent relation between the book which Johnson describes and the book which Ryan has written.

Ryan's principal thesis is that fatigue and efficiency cannot be satisfactorily evaluated in terms of performance measures alone. Performance, Ryan argues, can be affected not only by the adequacy of working conditions, but also by the effort which the worker devotes to his task. A well-motivated worker may often maintain a high level of output under very inadequate conditions of work as a result of compensatory increases in effort-expenditure, and for this reason conclusions based only on performance measures may be quite misleading. Ryan maintains that considerable attention must be given to the possibility of developing objective indices of effort and devotes several chapters to the status of research in this area. Yet Johnson writes:

> I differ from the author in his presupposing that decrement of performance measures, in general, the effectiveness of an agent of impairment such as prolonged labor, deprivation of sleep, deprivation of oxygen, or dosage of alcohol or drug. For the presupposition denies by implication—but yet effectively—that the living organism is a compensating mechanism, though it really is. . . . Therefore I have to regard part of Dr. Ryan's work as being maldirected (p. 176).

The extent to which Johnson has misrepresented Ryan's position may be judged from the following quotations from *Work and Effort*:

On noise. "A number of different lines of evidence suggest, however, that this adjustment to noise is attained at the expense of additional effort on the part of the subject" (p. 125).

On temperature. "One possible interpretation of these results is that the subjects were 'put on their mettle' by the experiment. Perhaps, then, they compensated for the effects of adverse temperature by increased effort, just as they did under the influence of noise" (p. 134).

On rest periods. "It is true that most of our knowledge of the effect of rest periods concerns output rather than efficiency. This indicates a gap in our knowledge which must be filled" (p. 151).

[1] RYAN, T. A. *Work and effort: The psychology of production.* New York: Ronald Press Co., 1947.

[2] JOHNSON, H. M. Review of T. A. Ryan's *Work and effort: The psychology of production. Psychol. Bull.*, 1950, 47, 174–177.

On methods of work. "The new pay rates may induce the worker to exert himself more in order to maintain or increase his earnings, and the additional production may not represent a true increase in individual efficiency" (p. 161).

On alcohol. "As Johnson has pointed out, the relationship of accident liability to alcoholic consumption is likely to be very complex. Johnson was referring especially to the tendency toward compensation. It is even possible that a driver could *improve* his driving performance after drinking, because he knows the danger and devotes himself to his driving with extra care and attention" (p. 262).

"Ryan strove for operational definition," writes Johnson (p. 174), and goes on to suggest that this attempt ended in disaster. At the outset of his book, however, Ryan illustrates the limitations of the operationa- approach and deliberately disavows it (p. 30). Johnson makes much of the fact that capacity cannot be measured directly. Ryan is fully aware of this difficulty but maintains, nevertheless, that the concept is meanl ingful. He writes:

> Capacity is thus a limit which is never reached in practice, and is a concept closely similar to the concept of *limit* in mathematics. Even though this limit is never reached, and therefore capacity cannot be directly measured, it can be estimated indirectly, and is a construct which is useful in our thinking about human activity, just as the concept of limit is useful in mathematics (p. 31).

Johnson's conception of compensation, by the way, bears not the slightest relation to an operational definition. Adopting his own standards of criticism, we might put to him such holier-than-thou questions as these: Compensation in what person? In what activity? At what time? How measured? The fact is, of course, that compensation, like capacity, is a concept which has meaning even though it cannot as yet be measured directly. And if we refused to give it a place in our thinking until we could measure it, we should never be led to undertake the experiments required to develop appropriate techniques of measurement.

Johnson's struggles with Ryan's formulation of the relation between effort, performance, and capacity afford a strange spectacle. Johnson introduces the assumption that performance may exceed capacity (which is contrary to Ryan's definition) and, after some totally unnecessary symbolic manipulation, emerges with the original assumption, which he only now recognizes to be contrary to Ryan's definition. Forgetting the source of the assumption, he concludes that Ryan's position has been shown to be "very awkward."

Several other brief comments by Johnson express disagreement with points which Ryan did not make. Nowhere, for example, did Ryan imply that the sum of two Poissonian distributions is itself Poissonian.

Nor was Ryan unduly critical of the work of Cobb and Johnson—he merely cautioned the student against unwarranted generalization from Cobb's sample and against *uncritical* application to accident data of techniques which involve the assumption of normality.

Johnson concludes that Ryan's book is "interesting and valuable" (p. 177). It is to be regretted that this evaluation tends to be lost in a wilderness of careless reporting and misdirected criticism.

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RYAN'S *WORK AND EFFORT:* A REPLY

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Ryan's definition (d) on his page 96, by which he usually tries to work, exhibits Effort as the ratio between one Power-Output and another. Now by definition, Power $= ET^{-1}$. Given the ratio P/P' between two determinations of Power, and expressing T, T' in terms of the same unit, such as the second, one readily sees that the ratio $P/P' = E/E'$. Hence, Ryan's definition sets forth Effort as a ratio between two determinations of Work-Output, the denominator term P' denoting Work-Output *capacity*. Now if P' was obtained by observation and measurement—that is, operationally—there is nothing in the definition to prevent any determination of P from exceeding it: that is, from turning out more work within a given time than he is capable of turning out. Of course this is merely a conversational necessity; but it is imposed on us by the name that we arbitrarily gave to the denominator-term in the definition.

Of course we could abandon the original definition and substitute a constant value for P', arbitrarily chosen so as to denote an output-rate that no subject could exceed. But the effect would be merely to substitute smaller units of measurement than the one originally employed.

Ryan does sometimes disregard his fundamental definition and substitute one that still implies measurability but which is less precise than the former. In doing so I think he does not improve his presentation.

Perhaps I ought to recall a peculiar form of *compensation:* the subject may shift his mode of attack in any of many ways; he may neglect some demands of his social environment in favor of the task in which his output is being measured. Thus he may maintain a normal

output until he is near to collapse. By watching for so-called clinical signs such as these one can learn much about *how* the subject compensates; but in so doing one will contribute nothing to the *measurement* of Effort in Ryan's sense of the term.

I did not say, nor imply, nor even insinuate, that Ryan had undertaken to analyze non-Poissonian distributions into Poissonian components, in the hope of getting generally useful results. My comment was *obiter*, but relevant to some attempts that have been made and published recently.

I do not believe that to ask an author such questions as Dr. Bitterman posed in his twelfth paragraph marks one as being "holier than thou," or holier than anybody else, or even perceptibly holy at all.

Certainly *I* do not refuse concepts places in *my* thinking because they imply non-measurability. I have long held that some existents are *intrinsically* non-measurable; and beginning with 1935 I have been running a series of short articles in the *American Journal of Psychology* on ways of treating such existents without falsifying them in attempts at quantification. I am sorry that they have been disregarded by some who might highly profit by studying them.

Received August 28, 1950.

A NOTE ON LOUTTIT'S REVIEW OF *INDIVIDUAL BEHAVIOR*

ROBERT F. CREEGAN
Ohio University

As a philosopher with a deep interest in the phenomenological movement in general, and in phenomenological psychology in particular, I desire to offer some comments on Professor Louttit's review of *Individual Behavior*,[1] a book which defends the phenomenological viewpoint in psychology. I will also offer a comment on the book itself. I have good reason to agree with the reviewer that few concepts of the particular book are novel. I also agree that attribution of (metaphysical) power to the phenomenal field was unnecessary. I am very much puzzled, however, by Professor Louttit's apparent assertion that the phenomenological concept violates the law of parsimony in psychology.

His argument is that "if the phenomenal field can only be inferred from past behavior, it hardly seems necessary to introduce the entity at all." We may agree that prediction concerns relationships between past behavioral data and future behavioral data. Yet it seems necessary to note a fact which really should be obvious. The data do not themselves predict anything. A psychologist is needed—that is, a thinking observer and systematizer of facts. The data must be reduced to logical order of some sort. Prediction bridges the gap between ordered data of the past and ordered data of the future. Experimentation deals with features of situations which are isolated with reference to ordering concepts. The experimentalist is not concerned with situations in the raw. If this be granted, may we not admit that the "phenomenal field" may be a useful ordering concept?

Phenomenological psychology asserts that, through the discovery of "personal" meanings of behavioral situations, and their "personally" meaningful parts, the data may be ordered in a useful manner for setting up experimental situations of the clinical and the educational, as well as of the more strictly laboratory types. Not every type of experiment is equally useful for every purpose, but the phenomenologist contends that such concepts as "client meanings," "student meanings," and the like, are useful ordering concepts for experiments on motivational development, intellectual development and related trends. Precisely how this would be done is another question. There seems to be no justification, however, for ruling out *a priori* the possibility of ordering the behavioral data in this way, or the possibility that doing so would be fruitful for some research areas in psychology. Ordering concepts do

[1] LOUTTIT, C. M. Review of Donald Snygg & Arthur W. Combs' *Individual Behavior: A New Frame of Reference for Psychology. Psychol. Bull.*, 1950, **47**, 170–171.

not violate the law of parsimony, unless, indeed, all systematic thinking is unparsimonious. Perhaps science itself is somehow an illegal activity. I do not think, however, that Professor Louttit desires to sponsor the philosophy of irrationalism or primitivism.

The writer is not disturbed by Snygg and Combs' basic methodology which, as Professor Louttit pointed out, is by no means original, but rather he is disturbed by the fact that their particular phenomenological system has led thus far to few, if any, new empirical conclusions in such a field as motivation. Their contention that "preservation and enhancement of the phenomenal self" is the master motive (or even the sole motive of the normal adult) is strictly "old hat" (I plead guilty to name calling). This theory of motivation seems quite unphenomenological to most of us who are familiar with the literature of philosophical phenomenology. Even Koehler's descriptions of "phenomenal requiredness," which is not always, or perhaps even most frequently, ego requiredness, is more adequate. In conclusion, I would like to raise the question whether ego change is sometimes "required," not in the sense of ego enhancement, but rather in the sense of ego adjustment to, reflection of, or expression of, that which transcends the phenomenal self on dimensions of value experience. Self enhancement may be a function of a field reconstruction which is not a mere response to "phenomenal threats." Or is a self-suspected personal insensitivity one of the chief "phenomenal threats"? The "phenomenal self" of Snygg and Combs seems to be much more threatened by deficiency in personal mastery than by deficiency in personal sensitivity. Perhaps their particular system of phenomenal psychology really is true to "statistically normal" motivation. One reasonable objective for phenomenal psychology, though, is precisely that of placing the statistical norms in a wider perspective! These norms do not reveal all that being a man means, has meant, or may mean. Neither the pathological nor the ideal orders of activity should be omitted by a phenomenological psychology. Some of us think that one of the chief merits of the phenomenological approach is that those who use it will be less tempted, than are "gut psychologists," to imagine that the statistical norms in any way represent viable goals of eduction, or of counseling, in the broadest sense of the terms.

Received April 2, 1950.

LETTER TO THE EDITOR ON PETERS' REPLY TO LEWIS AND BURKE*

C. J. BURKE
Indiana University

In connection with the discussion of chi-square in the July 1950 issue of the *Psychological Bulletin*, the paper submitted by Peters differs in an important respect from the copy to which Dr. Lewis and I wrote our reply. In our discussion on p. 348 we have a reference to Peters' third footnote. In his published criticism of our original paper, the footnote in question was omitted, with the consequence that our remark is left dangling. Further consequences are that our reply to Peters' point 3 on pp. 333–334 of his article is inadequate, since this reply was based upon the missing footnote. The omitted footnote is crucial for our reply and serves better than anything we could write to illustrate Peters' confusions with respect to the chi-square test. The omitted footnote, numbered 3 in Peters' original manuscript, reads as follows:

> There may be needed a re-examination of the applicability of Pearson's formula *vi* to the new conditions for which he did not intend it. He developed the formula on the assumption of freely floating marginal totals, only the total *N* being fixed. When R. A. Fisher, for the sake of using true values for a narrower universe rather than estimated ones for a broader universe, proposed constant marginal totals with a consequent change in the number of degrees of freedom, he does not appear to have re-examined Pearson's fundamental derivation under these new conditions. With freely floating marginal totals and only a limitation on the total *N*, a certain system of intercorrelations among the cells arose of which he took account. Under the new conditions a new system of intercorrelations would arise which might very well not be the same. A new derivation with the new assumptions might not come through with the same simple formula *vi*. I am not myself able to carry this through; but invite the attention of expert mathematicians to it.

Our comment on page 348 indicates that the derivation Peters requests is already available in one of the best known statistical sources and that

* *Editor's Note:* The editor is entirely responsible for the error which caused Drs. Lewis and Burke to criticize a footnote in Dr. Peters' original manuscript which he subsequently deleted from the galley proof of the article. Dr. Peters wrote to the editor before his paper was sent to Drs. Lewis and Burke for comment, and asked that the footnote in question be deleted. He stated that if the paper had gone to press he would take the note out of the galley proof. Through an oversight, the editor did not delete the footnote from the manuscript, nor did he read the galley proof of the article. Galley proofs and subsequent editorial work are now handled in the central office of the American Psychological Association.

he is mistaken in his notion that the degrees of freedom are changed.

Peters' comments on his point 3 are part of the same confusion. Without the footnote just quoted our reply does not cover the points he raises and adequate clarification would require considerable discussion. With the footnote, the discussion as given in our reply is easy and adequate.

Received August 18, 1950.

CHAPANIS, A., GARNER, W. R., AND MORGAN, C. T. *Applied experimental psychology.* New York: John Wiley & Sons, 1949. Pp. v+434. $4.50.

This is the first systematic text dealing with the relations between psychology and engineering—with problems of designing machines so that they are adapted to the characteristics of human beings. It is an important book because it defines the boundaries of this new field and indicates the problems that confront the psychologist who chooses to work in it. The title "Applied Experimental Psychology" was chosen because the research techniques employed most generally in studying equipment-design problems are the traditional ones of experimental psychology. The terms human engineering and engineering psychology are also used in describing the field.

In contrast to most psychology texts, this one is concerned almost exclusively with environmental variables rather than organismic variables. The central problem is, How can man structure his environment so that his job of observing, interpreting, and reacting to it can be done most efficiently? There is no discussion of such familiar topics as selection, training, individual differences, or motivation of workers. Instead, the authors are concerned with the tasks performed by workers and with designing the machines used by them so that the typical individual will perform his job most effectively. Heretofore this point of view has received little emphasis in psychology. Its exposition in the present book reveals many new possibilities for applying psychology.

After the introduction, the reader encounters two chapters on elementary statistics with illustrations chosen from problems of designing machines to suit human characteristics. This brief treatment of statistics will be of little value to the psychology graduate student. Perhaps the chief justification for the inclusion of this section is that design engineers and other readers who are relatively unfamiliar with modern psychology may be impressed with the fact that it is a precise, objective science. The most interesting problem dealt with in this section is how complex man-machine systems can be analyzed so as to identify the major sources of variable error.

The next six chapters deal with visual and auditory perception and with the applications of perceptual data to problems of designing devices for "displaying" information to the human senses. Two more chapters are devoted to the nature of motor skills and to the related problem of designing controls that are adapted to human motor abilities. The sections on "How We See," "How We Hear," and "How We Make Movements" provide a lucid and interesting reorganization of familiar psychological data. The unique sections are those in which

the reader is shown how these data can be applied to the design of mechanical and electrical equipment. Much of this material, such as the design of dials and scales, of signalling systems, and of tracking devices, will be new to psychologists. Most of it has become available to psychology as a result of research supported by military organizations during the last decade and carried out at such places as the Air Force's Aero Medical Laboratory, Harvard's Psycho-Acoustic Laboratory, and Johns Hopkins' Institute for Cooperative Research. The last three chapters deal with the arrangement of tools, equipment, and the work place, with work and fatigue, and with the working environment. Much of the material included here is typical of that found in textbooks in industrial psychology and in industrial engineering.

The authors have been very successful in finding ways to display visually the data of psychological experiments. They take seriously their own conclusion that "pictorial visual displays" are easier to interpret than are "quantitative visual displays," and use a total of 196 figures in contrast to only 13 tables as a means of summarizing psychological data. The art work is uniformly excellent and adds greatly to the readability of the book.

Nearly all of the experimental data relative to equipment design problems that were available at the time of publication have been summarized. However, in spite of the inclusion of a number of experiments of questionable importance, the reader is left with the conviction that there are vastly more questions than there are answers in this area of psychological application. A creditable job is done in defining and summarizing these problems, but the picture that the critical reader gains of the level of the systematic and theoretical work in the area leaves him somewhat dissatisfied. This, of course, is more a criticism of the status of work in the area than it is a criticism of the first systematic text covering this new field. In the same vein, it can be pointed out that much of the experimental work on equipment-design problems relates most directly to the design of military equipment, particularly to the design of airplanes, airplane instruments, air traffic control equipment, and radar.

One of the interesting features of the book is its style. The authors originally organized the material for a series of lectures on men and machines at the Naval Post-Graduate School at Annapolis. In amplifying and revising these lectures for publication in book form, they have carried over the notion that they are talking to the reader. They state in the Preface that they have " . . . made deliberate efforts to use small words instead of big ones; short direct sentences instead of long indirect ones; and colloquial expressions instead of academic and technical phrases." A few psychologists of the older school of writing may be somewhat disconcerted to read that "The brain is a mighty fallible instrument" and that "One of the biggest troubles with people . . . is that

they get tired." However, this reviewer thinks that most readers will find the style refreshing and will conclude that the material loses nothing of its effectiveness by being presented in small words and short sentences. One result of the style is to make psychological research sound rather easy and quite exciting. This results in part, however, from the fact that the authors have included a minimum of detail in describing experimental techniques and methods. The serious student of experimental psychology will find no precise descriptions of problems, of techniques, or of the exact procedures used in the statistical analysis of data.

"Applied Experimental Psychology" is good general reading. Many psychologists whose primary interest is in another area will want to browse through this book in order to find out what engineering psychology is about. For students with a minimum of training in psychology, such as students majoring in engineering, the book should provide an excellent text. For advanced undergraduates and graduate students in experimental and applied psychology, its chief value will be as a source of supplemental reading. For those who are seeking materials for the introductory course or topics for the experimental course that are related to problems that are of vital importance to people who work in today's technological society, the book will provide a rich source of ideas.

PAUL M. FITTS.

Ohio State University.

CAVAN, RUTH S., BURGESS, E. W., HAVIGHURST, R. J., & GOLDHAMER, HERBERT. *Personal adjustment in old age.* Chicago: Science Research Associates, 1949. Pp. xiii+204. $2.95.

This monograph has three purposes: "(1) to define and to analyze the nature, patterns, and problems of personal adjustment to aging; (2) to present the available facts about old age derived from census data and from a special survey of 2,988 older persons made by the authors; and (3) to describe two instruments for measuring adjustment in later maturity" (p. v). It contains a thoughtful discussion of definitions of aging, of the cultural patterns of old age groups, and of old age adjustment; provides a compact source for census information describing groups past sixty; and presents a number of excellent "sketches" of old people and their modes of adjustment. Although addressed to a heterogeneous audience (research workers, college teachers and their students, social workers, and the general public), in this reviewer's opinion the volume will be read mainly by the first two groups and should be evaluated primarily as a research contribution.

The original data presented in this monograph are of two kinds: (a) information from 499 men and 759 women, describing age trends

from the sixties through the nineties with respect to companionship, degree and type of participation, health attitudes, religious attitudes, and adjustment attitudes; and (b) data based on varying *N*'s up to almost 3,000 relative to the construction, validity and reliability, and interrelations of parts of an "Attitude Inventory" and "Adult Activity Inventory," the two being published together as *Your Activities and Attitudes* questionnaire.

The age-trend data provide the best information currently available on the aspects of personality studied, for the years past sixty. Although trends are reported through the nineties, it is somewhat disturbing to see percentages computed in the upper age brackets on *N*'s of 14, 6, 4, and even 2, although attention is called to the sample size in table footnotes. Such reporting is apt to lead to misinterpretation on the part of the non-technical reader, particularly in instances when percentages are given in the text with no *N*'s indicated. At one point (p. 59), the authors themselves mention with reference to "zest for living" that there is "some indication of increased zest for the very last age period," this comment being occasioned by a "reversal" in trend in the very last of eight or nine age categories produced by 33 per cent of three male cases and 100 per cent of two female cases! Although the data presented extend previous knowledge, the value of this report would have been enhanced if age trends for all items had been presented in an appendix. And a more adequate description of the sample (age for age) would have helped the reader judge the extent to which progressive selection had biased the trends reported, an important consideration since most of the findings are based on a 32 per cent return of a mailed questionnaire.

A more general issue relating to age trends deserves special comment. In an early chapter the authors consider at some length the definition of "old age," and recommend definition in terms of some arbitrarily selected percentage of decline. Although such a definition is not without merit, it presupposes an adequate body of information regarding peak years and change during the adult life span, if a reasonable base and percentage are to be fixed upon. Research limited to the age span of this investigation (and many other investigations are similarly limiting their research) not only fails to produce the necessary information regarding the years 20 to 60, but results in a *stage-of-life* psychology of old age, rather than a *developmental* psychology, despite verbal recognition of the latter in this and other publications as the more appropriate view. An example from the present investigation will illustrate the need for a broader age sample. It is noted that practically all the age trends are consistently downward from age sixty on. Yet it makes a great deal of difference, psychologically, whether these trends are continuations of trends originating at thirty or are trends that have *just begun at sixty.*

It is, of course, difficult to evaluate a report written for a heterogeneous audience. Research workers will wish certain information (e.g., in the instance of this report, more information regarding reliability of differences and other statistics) which would tend to clutter up the monograph for the non-technical reader. This reviewer has found the monograph stimulating and useful in courses dealing with the psychology of maturity and old age. It is especially rich in research suggestions, both explicitly stated and implicit in the discussion and findings, in particular in the interrelations among sub-sections of the inventory. The volume merits careful consideration as an important contribution to the growing field of gerontology.

RAYMOND G. KUHLEN.

Syracuse University.

BURROW, TRIGANT. *The neurosis of man.* New York: Harcourt, Brace, 1949. $7.50.

This book represents a recent comprehensive statement of theory and research as products of the Lifwynn Foundation where the author was scientific director. Brief review presents difficulties since the book is an exposition of a new system of behavioral explanation and analysis with its own peculiar terminology. Racial neurosis, phylopathology—an aspect of phylobiology—is thought to be the essential problem rather than the neurosis of the occasional deviant individual. Phyloanalysis, a special form of group and neurodynamic analysis, has discovered two phylogenetically conflicting systems within man that account for his maladaptive behavior. Of more remote biological origin is the tendency for man-in-the-group to react on a group or phyloörganismic basis with a minimum of self-reference and projection and with a maximum of objective group orientation. Of more recent biological origin, associated with the appearance of speech and the ability to utilize symbols, is the development of the "I"-persona, by which is meant "the artificial system of prefabricated affects and prejudices that underlies man's present level of 'normal' feeling and thinking and that leads to inferences that are lacking in biological warrant." Two varieties of attention have been found to be associated with these systems. The one, cotention, occurs when one relates himself directly and in a free functioning way to objects about him. This attitude or attentional trend harmonizes the individual with the phyloörganism. The other variety of attention, ditention, occurs when one is preoccupied with his interests, self-awareness, desires, affects, prejudices and ideas of self-advantage ("I"-persona). Cotention is thought to be phylobiologically integrative, while ditention is disintegrative. The development of the "I"-persona with its ditentive trends is essentially an autopathology that has led to a world or racial neurosis. Since we all share this autopathology, traditional psychiatry and psy-

choanalysis do little to alleviate the condition. The usual group therapies are similarly irrelevant.

Research records indicate a decrease in breathing rates, a decrease in eye movements, and altered brain wave patterns under conditions of cotention in contrast to records of these responses associated with ditention. Such data are taken to imply neurodynamic bases for these two contrasting phylogenetic systems.

The comprehensive implications of the system for man's welfare are indicated thus: "If insanity, crime, conflict, neurosis and war are to be remedied, . . . ditention and the spurious mood of the 'I'-persona must be made to give way to cotention or to the organism's balanced motivation as a phylic whole." The essential teaching of the book seems to lie in the direction of some kind of self-surrender to the group, the relinquishment of "partitive" self-seeking for individual rights. However, just what final social form or method should be achieved for expressing the ultimate racial non-neurotic norm in healthful, constructive, cotentive cooperation is not clear. The relationship of these implications to a totalitarian state, self-abnegating religions, and the essential philosophy of democracy should have further consideration.

REX M. COLLIER.

University of Illinois.

FLEMING, CHARLOTTE M. *Adolescence.* New York: International Universities Press, 1949. Pp. vii+262. $4.50.

In this compact book on adolescence, the reader will find all of the pertinent facts necessary for an understanding of the maturing individual, without being smothered in a welter of irrelevant and inconsequential detail. This is as it should be, since the text has been written for those who work with and for adolescents.

It is obvious that the author approached the problem with a deep understanding of modern youth. The book is well integrated and reads as if she developed her concept of adolescence and then marshalled the facts in support of her thesis. As a result there is a consistency of viewpoint that is lacking all too often in other texts on the subject. This viewpoint has two aspects: (1) the necessary adjustments to puberty and physiological maturation are determined primarily by cultural rather than biological conditions, and (2) adolescents cannot be treated as a homogeneous group because of the wide range of individual differences and because of the variability of human development. As an educator this leads the author to the logical conclusion that "it is wiser to attempt a progressive adaptation of tuition to individual differences rather than to spend time classifying according to ability and then upon mass teaching of supposedly homogeneous groups." A corollary of this is that education beyond the secondary schools should be widely diversified. In this connection, although Fleming recognizes the usefulness

of aptitude tests, she refreshingly does not hesitate to emphasize that the relationship of test scores to achievement is universally low. It is the first time in years that this writer has seen in print the statement that a correlation of .80 is of relatively little value in predicting the performance of an individual on one trait from his score on the correlated aptitude test.

The book is well written in an easy flowing style. Although this leads to a few generalizations to which some might take exception, the book is extremely well documented. References at the end of each chapter run to as high as five pages. After a foreword and an introductory chapter the book is divided into three parts. Part I contains eight chapters and is concerned with the physiological aspect of pubescence and the concomitant changes in needs and attitudes. Part II examines the variety and variability of abilities and capacities of the maturing human being. At this point the unsophisticated reader will likely be more than confused by the statistical concepts employed, but there is a short appendix defining the terms used. The third part is practical and applied. In it the author traces the steps necessary to help the maturing individual in the transition from sociological childhood to adult status and independence.

The question arises as to who will find this book useful. The dedication reads "to teachers, foremen, parents, neighbors, judges, doctors," and "to all who may meet adolescents anywhere." Certainly all of these will find a sound, unemotional, and constructive appraisal of that period between puberty and adulthood. It should also make an excellent text for a one-semester course in adolescence at the undergraduate level; this is especially true if the students are referred to the original sources. Undergraduates will miss the pretty pictures which are gradually replacing the "meat" in American texts, but they will get a well written evaluation of the best research on the subject. For the more mature student of the subject the book's greatest value lies in its extensive biliography of research on adolescence done in the United Kingdom.

RICHARD T. SOLLENBERGER.

Mount Holyoke College.

JOHNS, RALPH LESLIE. *Psychology in everyday living.* New York: Harper, 1950. Pp. xii+564. $3.50.

This is an attractive, readable, brief, simplified text for the first course in psychology. The sentences and paragraphs are short, many concrete cases are cited, and a glossary appears at the head of each chapter. At the end of each chapter, the author provides questions for discussion, lists of relevant films, self-tests, and suggestions for simple experiments, demonstrations, and exercises. These pedagogic devices

are utilized effectively to promote understanding and to heighten interest.

Consisting of but 458 pages of text, Johns' is one of a number of brief texts recently published (Johnson, Garrett, Guthrie and Edwards, Brown and Gilhousen) designed for a single semester. Although all the major topics of general psychology are here, yet accuracy and full understanding are sometimes sacrificed for conciseness. Thus we read that strabismus is "imperfect focusing" (p. 181), that in hyperopia the image is focused behind the retina (p. 180), that Binet's co-worker was Thomas Simon (p. 301), that likes and dislikes may be called "brain drives" (p. 87), and that Kuder "correlates intelligence and vocational choice" (p. 312). Tests of mechanical, engineering, business, and art aptitudes are listed as examples of intelligence tests (p. 318), and we are informed that these tests can yield IQ's—not directly, but "by correlation" (p. 318). A questionnaire titled "Have you got what it takes?" and devised by Oliver Hinsdell, who is identified as one who "develops" players for the "screen," is printed as a test of aptitude for motion picture acting (pp. 515–518); no references are made to validity or reliability. We are told that psychology has two divisions, *pure psychology* and *applied psychology*. Instances cited of investigations within the field of pure psychology include the work of Thorndike on transfer of training, Terman on the measurement of general intelligence, and Watson on the behavior of the neonate (pp. 7–8).

The ideal textbook integrates principles and applications: the applications motivate the student and explicate the principles for him; the principles organize his information and provide a rationale for the applications. The "popular" brief text is likely to skimp on basic scientific content relating to the nervous system, learning, intelligence, sensation, and perception in order to make room for "practical" advice on how to be happy though neurotic, how to be popular, how to study, how to choose a vocation, and how to pick a mate. It is cause for gratification that psychology has practical implications; but practice without theory is worthless, if not dangerous.

Can psychologists provide an adequate and meaningful survey of their science in a semester? Useful and informative though this text is, one examines it with this question constantly in mind.

HARRY RUJA.

San Diego State College.

STANDEN, ANTHONY. *Science is a sacred cow.* New York: E. P. Dutton & Co., 1950. Pp. 221. $2.75.

"To be read in a hammock on a hot summer afternoon" should be printed on the wrapper of this little book, for it is easy and refreshing reading with its colloquial digs at the pomposities of scientists. We can often laugh with the author and sometimes at him. Much that this

court jester at the Shrine of the Sacred Cow has to say is sound, even if not new, and some of it is silly; but all is said with good humor and an amusing style.

Standen, the chemist, knows and tells us on many pages that science means "describing facts," explaining them "by making hypotheses or theories, which must agree with experiments, preferably exactly and quantitatively." What he makes fun of, and rightly so, is "the dreadful cocksureness" of scientists. He admits that there are individual differences. "Though collectively they are a society of mutual backslappers, not all of them go in for scientific evangelism. It is mostly the scientific educators that do." He gleefully quotes a number of purple passages from the exaggerated claims of these educators of what science courses are supposed to do for the students, and then proceeds to tell how deadly dull such courses can be. He is a chemist and ought to know. And the scientists? "As advertising always convinces the sponsor even more than the public, the scientists become sold, and remain sold, on the idea that they have the Key to the Absolute."

Science, Standen quite properly insists, does not yield truth, but only probabilities; it is not "infallible and beyond criticism"; and it has "many important limitations." The best of sciences is physics, and the best things in it "are fully as admirable as the scientists claim them to be." And yet, he says, the teaching of physics is often a messy and unenlightening job. The remaining sciences "can all be arranged in order, going from fairly good through mediocre to downright bad": biology, psychology, and the social sciences.

Biology, Standen tells us, is a great collection of facts with a "vast mass of analogies" and sweeping generalizations. He cleverly puts his finger on the unfilled gaps between species.

Psychology, the more important part in the proper study of mankind, "comes from the Greek word meaning soul, and would be clearer, but not more elegant, if we called it soulology. There is nothing mysterious in saying that man has a soul. Think of yourself, are you your body? It is "your" body: the "you" that has the body is your soul. . . . The body has weight, the soul does not: this is a perfectly clear distinction between the two." Simple, isn't it? But the poor benighted psychologists do not see it this way because they "can't ask the right questions, and they have given up (to a greater or less extent) the best way we have of understanding men." The real trouble with psychologists is that "being fundamentally muddle-minded" and having noticed the "close connection between the body and the soul they suppose that they are the same thing. . . . All of them pride themselves on their ability to reason, and most of them have a tendency to be a little humorless. . . . The less scientific they are the better they are doing." Which best-seller may he be talking about?

According to Standen there are plenty of people to go to beside

psychologists "if you really want to understand human beings." They are the novelists and poets. He does not realize that he is quoting from some textbooks. If he should ever find out, he will have to abandon his own cocksureness and ask himself why some psychologists have said precisely the same thing. He might then realize that it is dangerous even for a chemist, "first cousin" to the physicist, to pass judgment on workers outside his own field.

The author is far from belittling all that has been done in psychology "which has a number of fine achievements to its credit: treatment of war-shocked soldiers, and far better understanding of some kinds of mental illnesses . . . aptitude testing, job analysis, psychology of advertising (sic), attention-getting techniques, and methods of 'motivating' other people. But for all the usefulness of these, the really important questions in human life are hardly touched upon at all by psychologists." You will get little help from psychologists if you want to know about "really important human qualities, such as goodness, or holiness," or the various virtues. I have to admit that the author's aim is getting better.

The social scientists come off worst. Many of them "have an aggressiveness and cocksureness hard to match, even among the psychologists." They are as busy as the bees collecting facts so that they can do some correlating. In order to be scientific they frame hypotheses and proceed to test them. "This can always be done and it makes anything at all sound wonderfully scientific. If you want to find out whether your pencil is in the top drawer of your desk, you first frame the hypothesis. This is always verbally possible, but it reduces 'hypothesis' to such a miserably feeble thing that all the high-tooted virtues of the scientific method have been squeezed out of it." A good shot.

There is one curious omission in this catalog of the sins of scientists from the exalted physicists through the mediocre psychologists to the lowly sociologists: Nowhere is the distinction drawn between scientists and technicians. In fields with well established techniques and beautifully equipped laboratories it seems easy to become a respected technician turning out article after article and perhaps even a textbook, without knowing too much about the nature of science. The need for an understanding of what science is about is greater in the less developed fields. It is barely possible that there is more striving for such an understanding in the "mediocre" and "bad" sciences than in the "admirable" ones. At any rate, mere technicians posing as scientists are to be found at all levels.

Writing a book—or a review of it—is as revealing as a projective test. Although there is no mention of Thomas Aquinas, his spirit pervades the book like a faint and delicate aroma. Witness the concern with truth, virtues, the soul, and Plato's higher knowledge. And rising to a climax towards the end of the book, the author declares: "The first

purpose of science is to learn about God, and admire Him, through His handiwork." The reviewer replies: The purpose of science is to describe nature without God. In doing so science has definite limits which are fixed by its methods. If scientists want to concern themselves with God they can do so only outside those limits.

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GUILFORD, J. P. *Fundamental statistics in psychology and education.* (2nd Ed.) New York: McGraw-Hill, 1950. Pp. xiii+633. $5.00.

Unlike many revisions of popular textbooks, Guilford's second edition stands out as a completely rewritten text. Although some of the chapter headings look familiar, the extensiveness of topical coverage is considerably greater than in the first edition.

The first eight chapters of the book are devoted to a thorough yet clear presentation of descriptive statistics designed primarily for the introductory course. Following this, three chapters devoted to statistical inference represent considerable improvement over the superficial coverage of this highly important topic in the first edition. Nevertheless, this portion of the book still leaves much to be desired in view of the increasing use in psychology and eductaion of such techniques as analysis of variance.

The remainder of the book supplies a much needed, up-to-date discussion of the logic and statistical theory underlying measurement and prediction. It is here that Guilford is at his best. Articles concerned with test reliability and validity, scaling procedures, and prediction problems have mushroomed forth during and since the war to an extent which makes it exceedingly difficult for most psychologists to keep up with the progress being made. The consistent and clear treatment of these topics should prove most useful to research workers and advanced students who wish an orientation in modern test theory.

Several minor points may prove disturbing to some readers. Snedecor's *F* Table is *not* applicable to the simple case of testing the differences between two standard deviations unless the levels of significance are adjusted from .05 and .01 to .10 and .02, respectively (p. 233). To test the homogeneity of the "within-set" variances in analysis of variance the highly questionable procedure of making an *F* test for the difference between any two sets of estimates is advocated (p. 257). No mention is made of Bartlett's test of homogeneity or similar tests useful for this purpose. The discussion of analytic procedures in a two-way analysis of variance problems is superficial and somewhat misleading, particularly in regard to the appropriate variance estimate to be used in the denominator of the *F* test when the interaction mean square proves significant.

Apparently in order to simplify the discussion of statistical inference,

Guilford has used the term "t-ratio" indiscriminately for both the critical ratio test and Fisher's t test. This may lead to some misinterpretation by the reader, as for example on page 278: "When there is 1 degree of freedom in a contingency table, chi square is equal to t^2." This is true if t is thought of as a normally distributed variable. On the other hand, if t has Fisher's "t distribution," the relationship is an asymptotic one as the sample size approaches infinity.

The generous use of timely data from recent psychological research to illustrate and clarify the application of statistics throughout the book is quite refreshing. In addition to a glossary of the more important symbols commonly used in statistics and a section consisting of simple proofs and derivations, the appendices contain four tables not in the first edition. The thoroughness of revision and clarity of presentation should make this book highly useful as a basic text for both introductory and advanced courses in statistical methods.

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